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THE PSYCHOLOGICAL REVIEW.

STUDIES ON THE TELEGRAPHIC LANGUAGE.

THE ACQUISITION OF A HIERARCHY OF HABITS.

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I.

THE PSYCHOLOGY OF AN OCCUPATION.

A field for research is offered in the psychology of occupations. The chief engagement of every one is the acquisition or exercise of one or another association of habits, such as constitutes skill in a game, trade, profession, language, science or the like. With a little license one may call all of these occupations. In mastering an occupation, doubtless the whole man is involved, body and mind, sensation and movement, thought, interest, imagination, will,—innumerable known and unknown aspects of our psycho-physical life.

It might be argued that such an affair is too complex for scientific treatment until we have done with more elementary things, the fusion of ideas, the psycho-physic law, the chemistry of the cell, or whatever may be still more elementary. In reply, it may be said that the history of science justifies the study of concrete facts, however simple or complex, whether or not the results can at once be correlated with other facts and theories. One studies microscopically, another macroscopically. One studies the chemistry of the cell, another tone sensations,

another comparative religion. A fact fixed at any point stands in its own right, throws light at once upon the less and upon the more complicated aspects of reality, and so does its share toward a future correlation of the sciences into science. The fashion of a time may run now to narrower, now to broader studies; but time justifies all work which meets its test, verifiability *ad libitum*.

Most psychological studies, doubtless with good reason, have dealt with abstractions. This is obviously true of the studies, earlier and later, on will, association, attention, etc.; for these 'faculties' are plainly not concrete phenomena of conscious life, but artificially isolated aspects of conscious life. It is no less true that in the later laboratory studies on the fatigue of a muscle, the reaction time in a silence cabinet, or the like, we are dealing with abstractions. The reacting man, muscle, or ganglion is, indeed, concrete; but when a given process in one of these is studied experimentally, the first and hardest task is just the isolation of that process from 'disturbing conditions'—that is, from the complex stream of life in which alone it normally occurs.

The best of these analytic studies, earlier and later, are invaluable to science and, in due course, to the conduct of affairs. Invaluable, but still far from sufficient, by themselves, either for science or for practical guidance. The scholar singles out of the complex processes before him, some general aspect (law) or some group of facts. He exploits one or the other precisely and systematically. Excellent! But too often the price of this precision and system is an absorption which makes him blinder than his neighbors to facts or laws that are in the processes concerned, but outside the range of his methods, and to the actual course of events in which all the facts and laws known and unknown are interfused.

This blindness to things before his nose, but out of the focus of his attention, is the disease-of-the-scholar. He assumes that the particular principle or fact which he has defined substantially determines the whole stream of life in which it belongs. He writes an essay on will, or studies the latent period of an excised muscle, and thereupon issues commands to the

public schools. Science is his debtor if he has developed any truth. Science has time to wait for the rest. But if he tries to put his learning to work, the realities which he has ignored will have their revenge.

However, it is easier to see the need of trustworthy concrete psychology than to supply the need. The actual concrete processes of life are, indeed, all about and within us, but in a bewildering tangle. Out of this tangle we are all forced to get some 'knowledge of human nature' so that we may live together. To our own insights in this direction we may add those of others, those of artists and other sagacious men, those sanctioned by the folk. In this way we build up a concrete psychology, each for himself, and by this we guide ourselves in dealing with one another. It is the dream of the scholar to supplant this lore of the folk by an array of knowledge equally concrete and practical, but immeasurably wider, more accurate, more systematic, and freer from personal bias. The dream is long in fulfilling. There are quick ways, but they lead to pseudo-science. Witness phrenology, physiognomy, graphology and the more precocious chapters in criminology. Such outcomes warn us that there is no profit in fleeing from studies which pay for their precision by being abstract, to studies which pay for their concreteness by being untrustworthy. Better any fragment of cerebral physiology which is true, though by itself unable to tell any one what to do, than a Science of Human Character which tells every one what to do, but is not true. It must be recognized that macroscopic studies are subject to the same tests as the microscopic. The essential test in both cases is verifiability *ad libitum*.

The best examples of psychological studies at once concrete and reliable are to be found in the literatures of comparative psychology, psychiatry, criminal and individual psychology. Here in the best cases we have pictures of the typical conduct of animals, children, melancholiacs, paranoiacs et cetera, which instruct us better than unscientific popular psychology can, what to expect and what to do in dealing with individuals of these sorts. To this group of studies the psychology of an occupation would belong.

It would be well worth while if we could discern in any one man the chief subjective effects of mastering an occupation. Learning the business has been his chief concern, his most thoroughly evolutionizing experience. It has been an affair not of weeks or months of forced laboratory practice, but of years, wherein the natural interests of life have constantly driven him toward levels of skill only to be reached under such stimulation. In the measure that he has mastered the occupation, it has mastered him. Body and soul, from head to foot, he has—or one may say he *is*—the array of habits which constitutes proficiency in that sort.

Can such a case be studied with profit to science? The probability that it can be is increased by the fact that an occupation leads many men toward the acquisition of the same set of habits. These men are scattered all along the way from apprenticeship to mastery. Many of them begin and quit after touching lightly and being lightly touched by the business. These dabblers and failures are highly instructive objects of study. Many others press on into some usable degree of proficiency. These men are colleagues not in name only, but psychologically and physiologically. They have similar knacks, or similar traditions of the trade, or similar habitudes of some kind necessary in their business. They know, as well as they know anything about themselves, what the main habitudes developed by their occupation are; and if the psychologist can find his way to the right questions, they can give a valuable introspective account of those habitudes. It may be possible in the case of some occupations to supplement such testimony by objective experimental tests. A few in each occupation become experts, and of these an occasional one becomes able to do easily and quickly what his lesser colleagues can scarcely believe possible. Such cases are, of course, hardest to understand, and may escape all definition. But it would surely be worth while to begin the study of the genius by following him along that part of his path which he shared with many others. We might in this way, at least, find the point where he disappeared. That would be something.

In a word, society has already made for us in each occupa-

tion a vast experiment in the development of habits. If we can make use of some of these ready-made experiments, if we can delineate the path or paths by which one travels toward mastery of an occupation, if we can discover and describe the characteristic stages of the progress, if we can do these things so that every detail of our work can be objectively verified by any competent scientist, and so that the outcome will be accepted as true by those who have mastered the occupation, this should prove not unprofitable work. It should supplement what analytic psychology can do for pedagogy and psychiatry; for it would portray the actual typical procedures of men in learning or in failing to learn. And it should supplement what analytic psychology can do toward developing the science of mind; for it would exhibit not theoretical syntheses of alleged psychic elements, but the actual syntheses which the science of mind must accept and explain.

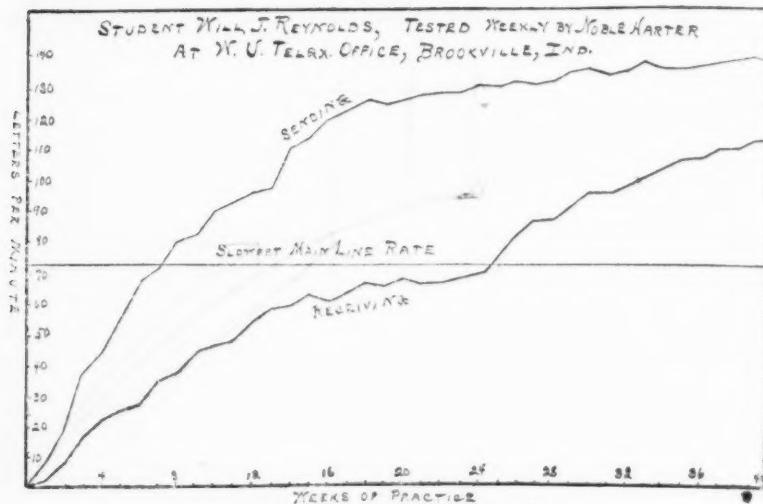
During the past five years the authors have made studies in the psychology of one occupation—telegraphy, utilizing throughout the work the experience of telegraphers as well as the methods of psychological research. The foregoing pages are not intended to overemphasize the importance of the results obtained, but to express a conviction which the study has developed, that in this direction lies a programme worthy the labor of many good men.

II.

DATA OLD AND NEW.

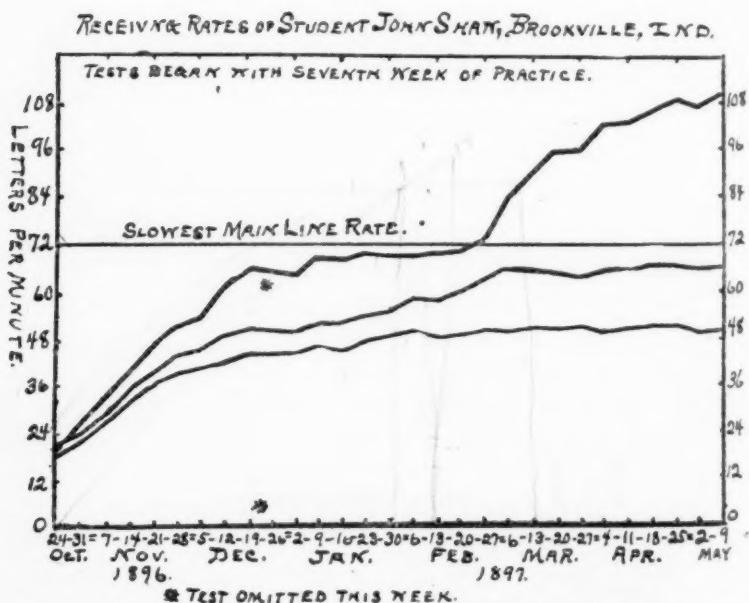
In a former series of studies on the physiology and psychology of the telegraphic language [PSYCH. REV., IV., p. 27] the authors gave the curves of improvement in sending and receiving. These curves were determined by the records of individuals tested each week, from the beginning of practice until fair proficiency was reached, and were confirmed by a consensus of opinion from about two hundred operators. As the conclusions of this paper are based in part upon those curves, one of the figures (X.) from the former paper is reproduced for convenience of reference.

Fig. X



Reproduced from PSYCH. REV., IV., 44.

Fig XI.



Connected discourse curve at the top; word curve in the middle; letter curve at the bottom.

The salient feature of the pictures shown in Figures II. to X., is the difference between the two curves. The sending curve has a form made familiar by many published practice curves. The receiving curve has for several months a similar form, but suddenly rises into what looks like a second practice curve. Moreover the history of expert telegraphers shows that after some years the receiving curve may ascend rapidly a third time.

Interest in the novel form of this curve deepens as evidence appears to show that it represents, in general, the course of improvement in various other acquisitions, *e. g.*, the learning of a foreign language, of chemistry, of English composition, etc. Interest is further challenged by the difficulty of explaining the form of the curve. In the former paper the authors proposed no explanation. None of our reviewers, nor of the psychologists with whom we have conversed, has given us a hint as to its meaning.

To investigate the problem further the following experiment was devised. A student should be tested each week on

- (a) rate of receiving letters not making words,
- (b) rate of receiving letters making words, the words not making sentences,
- (c) rate of receiving letters making words, the words making sentences.

These tests were made in the winter of 1896-1897. The subject was John Shaw, of Brookville, Indiana, who had begun the study of telegraphy about six weeks before the making of first test, Oct. 24, 1896. The method of making the test is described in *PSYCHOLOGICAL REVIEW*, IV., p. 48. The test was made each week until May 9. One test day, Dec. 26, was missed. The results are given in Figure XI.

Before discussing these results we subjoin evidence relating thereto derived from the introspections and observations of telegraphers. As hitherto noted (*loc. cit.*, p. 27), one of the authors (H.) was for years a telegrapher. To supplement his experience we have held long and satisfactory conversations with operators¹ of every grade up to the most expert men in the

¹ We cannot express too warmly our thanks to the members of the telegraphic profession for their cordial assistance without which the present study

country. We have asked telegraphers three principal questions :

A. To what is attention mainly directed at different stages of progress?

The answers agreed entirely, and were as follows: (*a*) At the outset one 'hustles for the letters.' (*b*) Later one is 'after words.' (*c*) The fair operator is not held so closely to words. He can take in several words at a mouthful, a phrase or even a short sentence. (*d*) The real expert has all the details of the language with such automatic perfection that he gives them practically no attention at all. He can give his attention freely to the sense of the message, or, if the message is sent accurately and distinctly, he can transcribe it upon the typewriter while his mind is running upon things wholly apart.

The feat of the expert receiver—for example of the receiver of press despatches—is more remarkable than is generally supposed. The receiver has two advantages over the sender. He can receive mentally far faster than any one can send; and with the typewriter he can transcribe much faster than any one can send. To bring the sender's rate up to that of the receiver abbreviated codes have been prepared. The receiver must translate the code into English words, and transcribe these correctly capitalized and punctuated, upon the typewriter. He takes, in this way, eighty or eighty-five words a minute. If mistakes are made by the sender, the receiver is expected to correct them as they come, and send a clean copy to press. The work continues for hours without leisure for re-reading, the pages being taken away to press as fast as they are finished. Yet, even during the performance of this astonishing feat, the operator is able at will to think about the significance of the despatches or to think of anything else he chooses. An Associated Press man, who has worked for years in one of our large cities, said to us: "I am in danger of allowing errors

could not have been successfully carried on. Especial thanks are due to Messrs. H. E. Jones, Assoc. Press, Cincinnati; Lot Lee, Assoc. Press, Indianapolis; Supt. Miller, Western Union, Cincinnati; E. B. Cassel, Chief Despatcher, Monon R. R., Bloomington, Indiana; and J. E. Sullivan, Chief Despatcher, Wabash Railroad, Peru, Indiana.

made by the sender to get into my copy, if I let my mind wander; but the truth is that in the last weeks, while taking press, my mind has been most of the time at home with a sick child."

B. How far can one 'copy behind' in different stages of his progress?

It should be explained that receiving is practically always 'copying behind.' That is, one does not, or should not, anticipate from part of a group of clicks what the rest will be; for if one guesses wrong, confusion of mind and error are likely to follow. Beginners are prone to guess ahead, and must acquire the habit of not doing so. Experts learn to wait. One expert said, "It is more natural to read back." He was asked if 'reading back' was like counting the strokes of a clock just after it is done striking. He replied, 'precisely.'¹

The answers to the second question were also concurrent.
(a) The beginner must take each letter as it comes, *i. e.*, he can copy behind one letter. (b) Later he can wait for words. (c) A fair operator can copy behind several words in connected discourse. (d) The expert prefers to keep six to ten or twelve words behind the instrument.

A count of the number of clicks (dots and dashes) in ten groups of ten words each, taken from a press despatch, gave the following result: 220, 275, 172, 214, 189, 267, 303, 260, 196, 281; average, 237.7. The achievement of the telegrapher in keeping correct hold of so long a series of sounds, and in doing this with a constantly changing series is, without doubt, one of the most remarkable feats of its kind. This is an example of a skill not to be reached by forced laboratory practice, but only by years of intense work.

C. What happens when you have to receive the disconnected words of a strange code or list of figures, such as bank clearings or the like?

The universal experience of operators upon this point was expressed by one expert thus: "When I get a word indicating

¹If, however, the first words of a very familiar phrase occur, they may betray even the expert into anticipating the rest of the phrase. This fact is a significant illustration of the subjective solidarity of phrases. See below, p. 364.

that a list of figures is to follow, I sweat blood until I can catch up." He said he could wait for six figures if they were in groups of three separated by a comma, but if the figures were isolated, he would want to be not more than three or four behind. In a word, he could hold in mind forty to sixty or more of the elementary groups of the Morse code, if these 'made sense,' but only three or four, if wholly disconnected.

Note on the Reading of the Blind. To get cross light upon some of the foregoing points, information was sought concerning the reading of the blind, from Miss Nellie Love, an expert teacher in the Indiana Institute for the Blind at Indianapolis. She reports as follows:

"(1) Upon what is the attention of the pupil fixed as he reads?

Upon first reading a new selection :

(a) In a First Reader class of twelve every one kept his finger on the letters, spelling each word either out loud or to himself.

(b) In a Second Reader class of eighteen the attention of all but three was upon the words. These three read to see what the story was about.

(c) In the Fourth Reader class of seventeen the larger number gave attention to the words; the others to the thought.

(d) In the next grade, a class of about the same size, more regarded the thought, only three or four the words.

(e) In the highest grades the attention was upon the thought, except when the words were unfamiliar.

"(2) How far does the pupil read with his finger ahead of his voice?

(a) In First and Second Reader classes, not at all.

(b) In Third and Fourth Reader classes, most pupils keep finger and voice together. Two report the fingers one word ahead.

(c) In the highest reading classes the majority keep finger and voice together. Several read three or four words ahead. One pupil, a very bright boy, keeps a line ahead, eight or ten words. He reads the end of one line with the finger of his right hand and at the same time reads the beginning of the next line with his left hand.

(d) In the advanced classes, where reading is not a special subject, the best pupils keep finger and voice together. In each class that studies reading as one subject, pupils who study each day, read and study the lesson, and then are able to read smoothly, rapidly, and several words ahead of the voice."

In all grades, sentences are read faster than disconnected words, and disconnected words faster than disconnected letters. The rates are not reported. All these results are closely analogous to those found among the telegraphers. Of course there are no blind children who have attained a proficiency corresponding to that of the expert telegrapher.

III.

CONCLUSIONS.

The immediate conclusions from the foregoing data will be given first; later (under IV.), an interpretation and discussion of these conclusions in connection with related literature.

1. *A Hierarchy of Habits.*

One might perhaps suppose that receiving telegraphic messages is simply transliteration or, at most, transverbalization from the code into the mother tongue. The operators reject this view. The evidence before us proves that they are right in doing so. Neither the letter curve nor the word curve nor both together, account for the receiving curve¹ except for a short period (see Figure XI.). Most plainly, the letter and word curves fail to account for the receiving curve where it rises rapidly from the plateau, while they continue their slight ascent. From an early stage some curve or curves associated with the combination of words in connected discourse must coalesce with the letter and word curves to give as a resultant the receiving curve. At the period when the resultant curve is rising rapidly, while the letter and word curves are rising slowly, the higher constituent curve (or curves) must be rising rapidly.

What does this higher constituent curve represent in the

¹The connected discourse curve in figure XI. will be spoken of as the receiving curve; its constituent curves, as letter and word curves respectively.

learner? Certainly not merely nor mainly increased familiarity with the meaning, structure or logical connection of sentences in the mother tongue. When, for example, the learner has rapidly shot up from a rate of eighteen to a rate of twenty-five words per minute, no one can believe that he has made this gain because of a sudden and enormous gain in knowledge of the language he has used all his life. All the facts point to the conclusion that the telegrapher must acquire, besides letter, syllable, and word habits, an array of higher language habits, associated with the combination of words in connected discourse. Mastery of the telegraphic language involves mastery of the habits of all orders. In a word, *learning to receive the telegraphic language consists in acquiring a hierarchy of psycho-physical habits*. For a discussion of this conclusion in connection with related literature see below, under IV., p. 360.

2. *The Order of Learning the Habits of the Telegraphic Language.*

The synchronous curves of Table XI. and the experience of operators agree in showing that from an early period letter, word and higher habits make gains (a) simultaneously, but (b) not equally.

(a) The simultaneity in these gains is shown in Fig. XI. by the fact that from the point where the curves diverge, each continues to rise. This is perhaps to be explained by the fact that from an early stage the learner practises with sentences, taking them as slowly as necessary. In this way there is incidental practice of every language unit and of every language unit in its proper setting.

(b) The curves of Figure XI. show also, however, that for many months the chief gain is in the letter and word habits, that the rate of receiving sentences is, in this period, mainly determined by the rate of receiving letters and words, and that rapid gain in the higher language habits does not begin until letter and word habits are well fixed. This objective result is supported by the introspective evidence of operators. In the first days one is forced to attend to letters. In the first months one is forced to attend to words. If the learner es-

says a freedom for which he is unfit, suddenly a letter or word which is unfamiliar explodes in his ears and leaves him wrecked. He has no useful freedom for higher language units which he has not earned by making the lower ones automatic. The rank and file of operators are slaves to the machinery of the telegraphic language. They must copy close. They cannot attend much to the sense of the message as it comes, but must get its form, and re-read for the sense. Only when all the necessary habits, high and low, have become automatic, does one rise into the freedom and speed of the expert.

3. *The Plateaus.*

We are now prepared to offer an explanation for the salient peculiarity of the receiving curve,—its plateaus.

A plateau in the curve means that the lower-order habits are approaching their maximum development, but are not yet sufficiently automatic to leave the attention free to attack the higher-order habits. The length of the plateau is a measure of the difficulty of making the lower-order habits sufficiently automatic.

(a) *The first ascent.* No plateau appears between the learning of letters and of words, because very soon these are learned simultaneously. However, as the letters are few, one is each week able to give more complete attention to the mastery of syllables and words as wholes. This perhaps accounts, in part, for the rapid progress of the first weeks.

(b) *The first plateau.* For several months the learner is compelled to attend almost exclusively to words. The number of words which he has to learn in order to receive whatever messages come, is great. The average amount of practice which each word receives is therefore small, and the increase in the average rate of receiving correspondingly slow. This very slow increase of rate we have called a plateau. It continues until the learner has the necessary vocabulary so well learned that he can have his attention free for something else.

Another retarding influence during this period is doubtless the learner's slight hold upon the higher language habits. The importance of this retarding influence in comparison with that

of an imperfect vocabulary, can not be determined without additional investigation.

(c) *The second ascent* represents the acquisition of a new set of language habits. This is *a priori* probable from the consideration that in practice curves generally rapid progress appears when the developing function is in an early stage. We are not, however, left with a probability. While the receiving curve is rising rapidly the synchronous word and letter curves are continuing their ascent slowly. We, therefore, *know* that the learner is gaining speed by taking in some way increasing advantage of word combinations. Part of the reason why he improves so fast is, doubtless, that he has already been unconsciously habituated for certain phrases and forms of word combination in the period when he was attending mainly to words. *It may be that the rapid ascent of any practice curve represents mainly a quick realization of powers potentially present by reason of preceding gradual and unconscious habituation.* With the increased ability in taking sentences there comes, without doubt, increased ability to take isolated words and letters; *but, as one improves, the three curves diverge more and more. This means that skill depends more and more upon the acquisition of higher language habits.*

(d) Only the first few months of the period during which one is a practical operator, but not an expert, have been investigated experimentally. Our knowledge of this period rests mainly upon the testimony of operators. Men of this rank, of course, vary widely in skill and in rate of improvement. There is, however, one essential point in which operators who are not experts are more or less alike. They are all, in some degree, tied to the mechanism of the language. They cannot copy far behind. The mind must not wander far from the incoming stream of words, even to dwell upon the sense of the words. Few operators ever obtain complete freedom in the telegraphic language. These few must earn their freedom by many years of hard apprenticeship. Our evidence is that it requires ten years to make a thoroughly seasoned press despatcher.¹

¹ We have shown above that receiving is not translating either letter by letter or word by word into the mother tongue, but involves the use of a great

(c) *The final ascent.* The testimony of experts is that the ascent from drudgery into freedom is as sudden as was the ascent from the first plateau.

Note on the Sending Curve.

Why does the sending curve have no such succession of plateau and ascent as appears in the receiving curve?

There is no plateau in the sending curve in the earlier part of its course, because, as in the early part of the receiving curve, the various habits involved are acquired simultaneously (compare page 357), and there is no sharp ascent later, even when one becomes an expert, because such an ascent is mechanically impossible. At all stages one has in mind plenty of words ready to be sent as fast as the motor habits will permit. At first one is learning motor letter habits. Soon, however, also motor word habits. The sending curve rises accordingly in a fashion analogous to that of the receiving curve in its early stage. By and by, however, a mechanical limit is reached. Sending is, at the best, a slow business. A letter or digit requires from one to six strokes. Spaces of various length must be allowed for. One cannot utilize both hands and several fingers, as with a typewriter. So, at less than fifty words a minute, a maximum has been reached that cannot be surpassed.

4. *Effective Speed and Accuracy.*

(a) *Effective Speed.*

It has long been known that connected words can be read faster than disconnected, and letters combined in words faster than disconnected letters.¹ The facts upon this point, old and new, justify, we believe, the following conclusion: *Effective array of higher language habits—that telegraphy is psychologically a distinct language, almost or quite as elaborate as the mother tongue.* This view is supported by the fact that so long a time and such intense labor are required for the mastery of telegraphy—an amount of time and labor which would, without doubt, make the same men equally expert in any foreign language.

¹ We dissent, however, from the view that it is only or mainly the logical connection in sentences which accounts for the rapid rate in reading them. We believe (p. 366) that there are mechanical habits corresponding to often recurring peculiarities of sentences. This is shown by the fact that a series of words making no sense, if skillfully arranged in familiar sentence forms, can be read far faster than a series of words taken at random, and almost as fast as words making sense. Almost, but not quite. A consciousness of the sense appears to be still one factor in the affair.

speed depends, in a relatively small degree, upon the rate at which the processes dominant in consciousness occur; in a relatively great degree, upon how much is included in each of those processes. For further discussion see below, under IV., 4. p. 374.

(b) *Effective Speed and Accuracy.*

The gain in speed made possible by adding mastery of the higher language habits to mastery of the lower, does not lead to less, but to greater accuracy in detail. We have found invariably that many more mistakes are made in receiving disconnected letters than in receiving, at a much more rapid rate, letters that form words; and that, in turn, many more mistakes are made in receiving disconnected words than in receiving, at a still rapider rate, connected discourse. The practical experience of the telegraph companies proves the same. Although mastery of the higher order habits thus helps the receiver to accuracy in details, it cannot supply his ignorance of details. If a word not in his vocabulary comes as part of a dispatch, he is very likely to get it wrong. If he is often found making errors of this sort, it is proof that he needs a more extensive and accurate telegraphic vocabulary. Such a man is trying to receive faster than he can. He is trying to gain speed at the expense of accuracy. This is not *effective speed*, as his superiors will quickly let him discover. For further discussion see below, IV., 4. p. 374.

IV.

DISCUSSION.

In the foregoing, we have given little more than a bare statement of results. In the discussion of these results, we desire, first of all, to give the plain meaning of the facts known to us. We shall, however, use entire freedom in suggesting a wider circle of interpretations for which the evidence is not made out. We have, however, no interest in any theory suggested, except to see it tried by facts and assigned its proper measure of probability.

1. *A Hierarchy of Habits.*

A man is organized in spots—or rather in some spots far more than in others. This is true structurally and functionally.

It is strikingly true of the various sense organs and their functions. No less of the various parts of the central nervous system and their functions. A man has some habits which are sporadic and isolated, some which are bunched together in loose groups (such as the outlay of skills which make one a carpenter), and then, some habits which are knit together into a hierarchy.

A hierarchy of habits may be described in this way: (1) There are a certain number of habits which are elementary constituents of all the other habits within the hierarchy. (2) There are habits of a higher order which, embracing the lower as elements, are themselves in turn elements of higher habits, and so on. (3) A habit of any order, when thoroughly acquired, has physiological and, if conscious, psychological unity. The habits of lower order which are its elements tend to lose themselves in it, and it tends to lose itself in habits of higher order when it appears as an element therein.

There is reason to believe that proficiency in chess, geometry, chemistry and the like, involves in each case the mastery of habits which are associated in some such hierarchical fashion. Leaving these slightly investigated fields, however, we turn to that of language. The proposition that a language exists subjectively as a hierarchy of habits, is supported by a considerable amount of evidence scattered through recent psychological literature. This proposition is by no means identical with the obvious truth that a language is, objectively considered, a system composed of various units—letters, words, sentences, etc. The existence of the objective system is evident to all who know the language; the existence of a corresponding system of subjective habits demands proof. Is there, for example, a psycho-physically unitary habit corresponding to a familiar word, or does the recognition of a word involve the separate recognition of each letter? The latter view has been held. It requires convincing evidence from experimental psychology and psychiatry to prove that the recognition of a word is ‘eine gesonderte Funktion.’ In like manner it will require evidence not yet fully forthcoming, to show what higher language units and what characteristics of spoken and written language (*e. g.*, cadence, sentence-length, etc.) are represented subjectively by distinct habits.

(a) *Letters*.—A letter (printed or telegraphic) presents to sense a manifold. Recognition of the letter and recognition of its elements are distinct functions. One may recognize the dash and the dot of the telegraphic code after a little practice, and may *know* that $J = -\cdot-$, without being able to recognize that group of clicks when heard. To recognize the group as a whole with maximum rapidity requires weeks of practice. On the other hand, one may recognize a letter as a whole—for example, in Old English type—but be wholly unable to reproduce in memory the essential parts of which it is composed.¹

(b) *Syllables*.—Höpfner, in his study ‘Ueber die geistige Ermüdung von Schulkindern,’² finding that word errors are more frequent than syllable errors, and that letter errors are more frequent than errors as to parts of letters, remarks: “Silben” sind im Wort und Buchstabenteile im Buchstaben fester gefügt als Wörter im Satz und als Buchstaben im Wort. Wörter und Buchstaben sind also ‘selbständige’ Elemente.” }?

This observation is doubtless correct. Syllables are, however, sufficiently ‘independent’ to make it worth while for primary teachers to use the child’s stock of known syllables in teaching new words. Mr. Harter is of the opinion that a learner of telegraphy pays little direct attention to the syllables as such, but is really helped in the hearing of new words by the presence of familiar syllables.

(c) *Words*.—A child or one suffering partial aphasia, may recognize the letters of a word, but not the word as a whole. See, for example, the case reported by R. Sommer,³ who concludes: “Die Verbindung von Lautreihen zu Wörter ist eine gesonderte Funktion. Ein ‘Wort’ ist schon deshalb nicht als ‘Lautreihe’ zu betrachten.” On the other hand, children are frequently taught to recognize words as wholes before they know the letters of the alphabet. Decisive proof that the recognition of a word does not consist in the successive recognition of its letters, is afforded by Cattell’s result⁴ that a familiar word can be re-

¹ See Goldscheider and Müller, *Zur Physiologie und Pathologie des Lesens*. *Zeitschrift f. klin. Med.*, Bd., XXIII., s. 131–167 (1893). Reviewed by Walaschek in *Zeitschrift f. Phys. und Psych. d. Sinnesorgane*, VII., 228.

² *Zeitschrift f. P. und P. d. Sinnesorgane*, VI., 217.

³ *Zeitschrift f. P. und P. d. Sinnesorgane*, V., 318.

⁴ *Phil. Stud.*, II., 647; III., 470.

cognized in almost the same time that it takes to recognize one of its letters. This abundantly verified result one of the writers has found true of many children who are in their second school year.

Analogous facts appear on the motor side. One may be able to produce the separate sounds of a foreign language with considerable accuracy, as Karsten points out,¹ and still may not be able, without additional practice, to pronounce words. On the other hand, we pronounce the words of our own language with ease, but require special practice to produce the elementary sounds composing them. Karsten puts the matter thus :

(3) Nach dem oben gesagten wird man nicht einwenden wollen, dass, wer das bewegungsgefühl für das ganze hat, auch das für die einzelnen theile besitze und umgekehrt. Durch das erinnerungsbild ist eine bewegung von anfang bis ende abgegrenzt, dauer und art der mitwirkung aller in betracht kommenden organe fest bestimmt. Zwar können wir eine bewegung absichtlich an irgend einem puncte abbrechen, aber diese abgebrochene bewegung ist dann eben nicht mehr dieselbe, sondern eine andere, welche bei genügender wiederholung ihr eigenes erinnerungsbild entwickelt. Die bewegungen des arztes beim operieren, des malers, des musikers sind mechanisch und räumlich alle enthalten in den einem jeden von uns geläufigen bewegungen ; doch gehört übung, das heisst ausbildung der bewegungsgefühle dazu, um gerade eine bestimmte bewegung genau auszuführen. Auch kann man eine bewegung, die man z. b. mit fünf fingern leicht macht, nicht sofort mit einem oder zwei fingern nachahmen ; das wäre zwar ein theil der früheren, aber doch auch eine bewegung für sich, für die das bewegungsgefühl erst eigens entwickelt werden muss.—Kurz das bewegungsgefühl kann etwas einheitliches sein, auch wenn die wirkliche bewegung compliciert ist, und einheitliche bewegungsgefühle für grössere lautgruppen können in der seele sich bilden getrennt von denen für die einzelnen theile, aus welchen jene gruppen bestehen.

(d) *Word groups.* As certain letters often appearing in the same order give rise to a unitary word habit, so several words often appearing in the same order give rise to a phrase habit. Such word groups sometimes come to have a unity almost equal

¹Sprecheinheiten ü d. Rolle in Lautwandel ü Lautgesetz; *Proceedings Mod. Lang. Assoc.*, Vol. III., 1887, p. 3.

to that of single words. As a rule, doubtless, the fusion is not so close; that is, we pass more easily than in the case of words from the consciousness of the whole to the consciousness of the parts. Nevertheless, the tendency of the first part of a familiar phrase to suggest the rest,¹ and the fact that everyone has not only a characteristic vocabulary, but a characteristic outlay of word groups, show that phrases exist subjectively as unitary habits. Furthermore, it has been shown that one who reads a language with a certain skill is liable to make phrase errors as distinct from letter or word errors.²

Paul³ points out that we have many word groups (*e. g.*, *auf der Hand liegen*) in which a word has ceased to be associated with its ordinary meaning, in some cases (*e. g.*, *das Bad australien*) so completely that it requires a knowledge of the history of language to explain the connection between the meaning of the phrase and that of the individual word. In such cases, the language unit dominant in consciousness is evidently the phrase and not the word.⁴

(e) Habits Corresponding to Characteristics of Words, Phrases, etc. The language habits so far noted are specific *i. e.*, in each case a specific stimulus (letters, syllable, word or group of words) leads to a specific reaction. It is, however, a fact of the highest importance that one's stock of specific habits contains the material for innumerable other specific habits (and also, some hold, for 'generic' or 'plastic' habits). When one has learned *bat*, *cat*, *many*, *model*, one has four specific habits; but one is within two steps (which may be taken in a breath or only after deliberate pains) of a new habit corresponding to *mat*. The first step is dissociation (in the manner described by Martineau and James⁵) of the *at* from the first two words, and of the *m* from the second two; the second step is the fusion of these dissociated habits, when they appear in the order *m-at*, into one new specific unitary habit correspond-

¹ See case mentioned above, p. 353.

² Cf. Berger: *Ueber den Einfluss der Uebung auf geistige Vorgänge*, *Phil. Stud.*, V., 175.

³ *Principien der Sprachgeschichte*, 2 Aufl., 83.

⁴ Cf. Cattell, *Mind*, XI., 64.

⁵ James, *Psychol.*, I., 484.

ing to *mat*. (There is something arbitrary in the designation of *two* steps in the making of a new habit out of old ones. To ordinary introspection the process seems to have many steps when it occurs slowly and painfully, and only one step when it occurs in a flash, as when we recognize and adopt in an instant a new slang word—*mugwump*, *popocrat*. The words dissociation and fusion only designate and emphasize two essential phases of the whole process which ends in a new habit.)

In like manner, one's acquisition of these four words is partial preparation for *met*, *bet*, *cad*, and also for *bonnet*, *calico*, and for every word containing any syllable or letter learned. Further, the trochaic rhythm of *many* and *model* may become dissociated from these words, and may reappear as an aid in learning other trochaic words.¹

In the same manner, any element or characteristic of a word group habit may become serviceable in the learning of new groups. Doubtless, the primary effect of using a given word group is to establish a quite specific habit. One can re-read a sentence more quickly than one can read a new sentence containing the same words in a different order. One can even re-read a sentence more quickly if one follows the rhythm first used. The dissociation of language elements from the specific wholes in which they have occurred, and their use in the construction or understanding of new sentences, are a task—perhaps the most remarkable task of which men are capable. The stupider or lazier one is, the less one has inclination or power for this task. But even the stupidest and laziest man meets, with some measure of success, the conversational emergencies that confront him. From his small language capital, there rise substantially the right nouns, verbs, phrases, but's, if's, not's, and even the right inflections to denote the attitude and temper of his mind; and these elements fall together with amazing swiftness into sentences never before used by him. One who has genius for expression differs from the dullard in having a larger language capital, greater facility in dissociating the elements and characteristics, and greater facility in making new combinations. Until we have had a great deal more research

¹ Müller und Schumann, *Zeitsch. f. Psych. u. Phys. d. Sinnesorgane*, VI., 280f.

in regard to the higher language habits, conclusions in respect to them must be proposed with reserve. At present the following points seem probable:

(a) It is well known that the average *length of sentence* is characteristic for a given author. In most cases, perhaps, the author is unconscious of his sentence-length habit.

(b) A *rhythm* often used probably becomes habitual, apart from any particular words, and is then an aid in reading and a factor in making new phrases, sentences, and paragraphs, having that rhythm.

(c) A certain *order* of the parts of speech (*e. g.*, 'he walked out of the way,' or 'out of the way walked he') often recurring becomes habitual, determines the making of new sentences, gives us a sense of ease in reading straightforward prose, and a sense of shock at sentences like Browning's 'Irks care the crop-full bird? Frets doubt the maw-crammed beast?'—even when, as in this case, the words are all familiar.

(d) A *grammatical construction* often used to express a certain feeling (of plurality, futurity, doubt or the like) comes to be automatically associated with that feeling, apart from any particular sentence, so that either instantly and effortlessly suggests the other, to serve as one of many elements in the reading or making of a new sentence.¹

In like manner we may suppose that every peculiarity of style up to the structure and tone of a volume, corresponds to a more or less perfectly fixed habit. An E. P. Roeish novel betrays in its author a habit on its way to becoming as specific as sneezing.

Note on the development of new habits out of old ones. The old theory that doing particular things gives 'general training' of body and mind is nowadays confronted with the view that there is no such thing as 'general training'. The two views are perhaps not so irreconcilable as they appear to be in current psychological and educational discussions. The chief subjective effect of an act is doubtless its tendency to establish the habit of repeating that act; and, conversely, the best way to

¹ For the discussion of the point whether grammatical habits are specific or plastic, see below.

acquire skill in a particular act is to practise that, and not something else. But every bodily or mental process involved in an act is practised, and through dissociation and reassociation may appear in innumerable other actions. In the case mentioned above (p. 364), the 'fringes' of emotion and intention when the four words were learned tend to reappear upon repetition of these words; but may also, because of their exercise then, come up to reinforce the set of mind in a subsequent attack upon the multiplication table or the woodpile. When a boy drives the last nail in a fence as carefully as the first he is not thereby made ready to build a house, nor to codify the law of the commonwealth, nor to do anything else in the world so well as to drive nails into that fence; but his skill in nail driving will reappear when he undertakes carpentry; and the set of mind with which he drove them will reappear when he is a lawyer. We may deny that Grant's study of algebra gave him a general training of the mind that prepared him for the Wilderness, or for anything else so well as for that algebra, and nevertheless see that the mood of his hours with the algebra came up in his 'We'll fight it out on this line if it takes all summer.'

Professor Royce suggests¹ that besides specific habits one acquires generic or plastic habits, which lead not to a specific reaction upon a specific stimulus, but to a certain sort of reaction upon a certain sort of stimulus. He mentions especially the habits corresponding to the rules of syntax as in this sense generic. This view is attractive, and may be true. It may be, however, that there is no such thing as a plastic or generic habit, except in the sense that a habit may enter as an element into many different processes. Whether or not there are generic habits involved in the origination of higher mental processes, we believe that all habits tend to become in the same sense specific. ✓

2. *(The Order of Acquiring Habits which Constitute a Hierarchy.)*

Every one knows that, in general, habituation in certain actions leaves us free for others. This principle is, however empty and useless in a given field until we know what habits

¹ PSYCHOLOGICAL REVIEW, V., 118; EDUCATIONAL REVIEW, VI., 212.

are to be learned there, and which of these must be learned first, which second, etc. It is highly probable that in geometry, chemistry or whist one must acquire a hierarchy of habits; that some of these habits should be learned before others; and that some of them may with advantage be acquired simultaneously. Perhaps the most expert men have already felt their way to the right methods; but psychology and pedagogy would be greatly enriched by explicit and verifiable knowledge upon these points. Such knowledge the general principle stated above is impotent to give. It can only tell the student to do first things first. To discover what things *are* first in any particular field requires painstaking investigation, or a consensus of the practical experiences and intuitions of those who work in that field, or both. Though no one can foresee the results of such investigations in any particular case, there will be idlers in the psychological market place, when the results appear, ready to say: "Nothing new. We have known all along that some things must be done before others."

In point of fact, teachers of reading are not agreed as to the best order of studying the various language units. The older custom was to learn first the letters, then many syllables, then many words, and then at last to read sentences. In details this method varied widely; but its essential principle was to master lower units first and use these in picking out the higher. The newer custom is based upon an opposite principle. In the 'word-method' the pupil is taught a word as a whole before he knows any letter. In the 'sentence-method' the pupil is confronted with a short sentence before he knows any word or letter. In the later methods the subsidiary language units are to be learned incidentally, while the main attention is given to the higher language units and to the sense.

It is proved possible to learn to read by the older or the newer methods, and, indeed, by any method which brings the pupil for a long enough time into contact with print. The mind will find a method of its own. We believe, however, (1) that by no device is it possible to gain freedom in using the higher language units until the lower have been so mastered that the attention is not diverted by them; and (2) that it is, neverthe-

less, wise at all stages to practise with the highest language units possible, and thus learn all the units in their proper setting.

The alphabet-spelling-book method makes sure of the first requirement, but is grossly wasteful of time in postponing reading exercises which involve simultaneous practice of all the language units in their proper setting, and which are constantly more profitable because more interesting. The new synthetic methods gain these advantages, but lose a more necessary one, unless the teacher realizes that the pupils must all the while be getting the alphabet and vocabulary and making them automatic. If this end can be achieved incidentally, well and good. If not, it must be achieved by periods of practice devoted thereto. In no case can making the language elements automatic be skipped.

Similar principles hold in arithmetic. It is a mistake to demand of children a thorough memorizing of the number series and of the fundamental tables before giving them any exercise with concrete numbers and problems. It is a greater mistake to spend the years when the plastic memory is at its best in number exercises which are interesting, but which leave the children with the alphabets of arithmetic imperfectly mastered. The high-school boy who must halt in his mathematical work to remember the multiplication table, is enjoying the fruits of a pseudo-freedom in the grades. *There is no freedom except through automatism.* It is possible to avoid both the extremes mentioned. The work should be filled with concrete interest in ways fully displayed in our modern elementary text-books on arithmetic. But at all times the teacher should see to it that there is thorough incidental practice of those number-relations which should become automatic, and at some times there should be direct hard work at memorizing those relations.

Handwriting
including
mathematics
and
literature

In addition to the evidence already presented in favor of the foregoing view, two general considerations are submitted.

(1) It is quite useless to raise the question whether or not children should acquire specific automatic habits. There is no escape from such habits except by death. The Indian does not escape. The wolf does not escape. Neither Shakespeare nor Caliban escape. There is no question of escaping automatic habits. The only real question is: Which ones shall we acquire?

The school and civilization answer: While it is possible, acquire those habits which are the alphabets of learning and of cultivated life. This is the first necessary step toward the freedom, adaptability, ingenuity, and efficiency which give superiority to man.

(2) A school method must be judged by the moods and tempers which it cultivates, not simply by what is learned, still less by the momentary interest it arouses. If one forces mastery of the multiplication table by methods which keep one-half the school cowed and the other half rebellious, one has obtained a useful result at disastrous cost. Better not know the multiplication table than be thus morally maimed.

If, on the other hand, one anxiously converts all school work into a round of entertainments, if one shields the pupils from having at any time a sense of resolute effort with hard tasks, if one keeps the pupils vibrating between excitement and ennui as at a circus or picnic, what of the moods and tempers thus cultivated? To what set of character do they lead? For what occupation do they prepare? Every one knows. These are the moods and tempers of the loafer, the tramp, the sport—the idlers, rich and poor, who afflict society with their inefficiency and their consequent misery.

There is happily no need to choose between the galleys and the circus as models for the school and home. There are many schools and homes where hard tasks are performed in a good temper; where thorough drill does not arrest, but prepares the way for higher development; where children begin to do what they must later do to succeed in any business—pass cheerfully from interest in desired ends to a resolute drudgery necessary for the attainment of those ends.

If this view of education is correct, the course of study has no more important function than to make clear the essential habits involved in the mastery of each school subject, and the order in which these are to be acquired; and the teacher has no more important duty than to arouse in children such an interest in some higher aspect of the subject, that they will willingly lend themselves to mastery of its details.

*This does not hit
energy: he has an
answer.*

3. Plateaus.

Wide variation and sudden changes in rate of progress are not peculiar to the learning of telegraphy. In general, it is indeed *a priori* highly improbable that the rate of change in any process will be constant. For such constancy requires an extremely improbable constancy in the many factors which unite in determining the rate. As these factors increase in number and complexity, the less likely they are to effect a constant rate. Modern evolutionary science has emphasized the facts which indicate that changes in nature are regular and gradual. *Natura saltum non facit.* It is, however, now well-known that nature does make leaps. It may even be that saltatory change is the rule. The recapitulation theory invites us to picture the history of each individual as a series of steps corresponding to the stages in animal and racial evolution. No one has made out an accurate time table for all these steps (or even ascertained exactly what the steps are). But no one would claim that the rate of progress through them is uniform. The development of the body and the mind both show 'resting periods' alternating with periods of rapid change. We 'perch and fly.' We live for months or years upon a certain level of interests, efforts and achievements, and then suddenly undergo a more or less radical conversion. All things are become new. The old life sinks into the vast subsoil upon whose surface, for a season, bloom new forms of the life of attention.

The well-known examples of rapid change are, of course, not cited as specifically analogous to the plateaus and ascents of the telegraphic curve, but only to show that such alternations of camping out and moving ahead are not exceptional or abnormal. For specific analogies we must look to the history of analogous acquisitions. In this promising field for research nearly everything remains to be done. Preliminary inquiry has developed the following provisional results.

(a) *Languages.* As hitherto noted,¹ in learning to read (first year primary), and in learning a foreign language, one's progress is analogous to that of the student of telegraphy. In

¹ Loc. cit., 52.

the latter case, especially, there is the same rapid improvement at first, the same dispiriting level just below the ability to understand ordinary conversation, the same rapid ascent into usable knowledge of the language, and the same year long struggle, seldom completed, before one has freedom in the language.

(b) *English Composition.* In the Indiana University, we have each year several hundred students in conditioned English Composition. All entering students are tested as to their ability to write printable English. Those who cannot do so, are required to take the conditioned English until they can meet the test. A student may pass out of this work at any time. The heaviness of the work, the discredit of having to take it, and the special fee required, make the motives for getting through very strong. The instructors in this work tell us that the progress of most students is pictured in a general way by the receiving curve. A few students pass out of the work very soon. This generally indicates that they failed to do themselves justice in the first test. In most cases, there is rapid progress nearly up to the passing level, and then a long plateau above which the student seems incapable of rising. In some cases, where students were expected by the instructor to pass in a few weeks, they have kept drudging away for the rest of the year with slight improvement. Doubtless, in these cases, the interference of established language habits is an important factor in retarding progress.

(c) *Chemistry.* Several teachers of chemistry have reported that the progress of students during the first year's work in that subject is similar to that of the telegraphic student. There is the same period of rapid improvement in the first months, followed by a long period of slow progress. In the Indiana University chemical laboratory the latter period has long been recognized and named 'the period of depression.' At one time it was supposed by the instructors that this period of depression might be due to an inferiority in the latter part of the laboratory manual, but further experience has shown that this is not the case. An explanation of the chemist's plateau analogous to that given for the telegrapher's plateau would be: that on the plateau the learner is constantly hampered because

he cannot, on demand, remember any one of a large number of elementary facts which he has once learned; that the large number of elementary facts which he needs to know, makes his progress toward sufficient mastery of them very slow; that a rapid progress comes at last when he can turn his attention from mastering the elements to a freer use of these facts in attacking more complex chemical problems. The chemists whom we have consulted incline to regard this explanation as correct.

(d) *Miscellaneous.* A large number of individuals have reported analogous experiences in learning mathematics, music, whist, chess, checkers, et cetera. In all these fields we find one or more long discouraging levels, where practice seems to bring no improvement, ending, at last, in the case of those who persevere, in a sudden ascent. It is probable that in each case one must acquire habits of lower and higher order, and that the explanation for the telegraphic plateaus is the explanation for the plateaus in these fields. Of course, the curves in these widely differing fields must have different specific characters. Each must be investigated for itself. In a time when some fear a dearth of significant problems for psychological research the prospect of such a field is inspiring.

In general, we have here a point of view from which we may discern a difference between the master and the man of 'all-round' development, who is master of nothing. Both have, from the informal experiences of life, some knowledges and skills which fit them to undertake the mastery of a given field. Both have developed these potential instruments of mastery, have 'gone over' the principal items of knowledge and 'gone through' with the principal forms of skill required. The master has not stopped here. He has initiated himself body and soul in the elements, so that after a time such things are to him like letters and words to an educated man. They shoot together easily into new combinations. They are units of meditation, of invention. Meanwhile, to the man who has only 'a good general knowledge of the field,' the feats of the master are impossible and almost incredible. The master's units of thought are each to him a problem. He must give time and pains to each one separately. He cannot think with them. He

is necessarily a follower, or, if he essays the freedom without the power of the master, he is worse than a follower—a crank.

✓ 4. *Effective Speed and Accuracy.*

There is scarcely any difference between one man and another of greater practical importance than that of effective speed. In war, business, scientific work, manual labor and what not, we have at the one extreme the man who defeats all ordinary calculations by the vast quantity of work he gets done, and at the other extreme the man who no less defeats ordinary calculation by the little all his busyness achieves. The former is always arriving with an unexpected victory; the latter, with an unanswerable excuse for failure.

It has seemed to many psychologists strongly probable that the swift man should be distinguishable from the slow by reaction time tests. For (*a*), granting that the performances demanded in practical affairs are far more complicated than those required in the laboratory tests, it seems likely that one who is tuned for a rapid rate in the latter will be tuned for a rapid rate in the former, when he has mastered them. Moreover (*b*), a rapid rate in elementary processes is favorable to their fusion into higher unitary processes, each including several of the lower. Finally (*c*), a rapid rate in elementary processes is favorable to prompt voluntary combinations in presence of new emergencies.

In face of these *a priori* probabilities, eleven years' experience in this laboratory (the first three being spent mainly on reaction times) has brought the conviction that no reaction time test will surely show whether a given individual has or has not effective speed in his work. Very slow rates, especially in complicated reactions, are strongly indicative of a mind slow and ineffective at all things. But experience proves that rapid rates by no means show that the subject has effective speed in the ordinary, let alone extraordinary, tasks of life. How is this to be explained?

The following answer is proposed: The rate at which one makes practical headway depends partly upon the rate of the mental and nervous processes involved; but far more upon how

much is included in each process. If A, B and C add the same columns of figures, one using readily the method of the lightning adder, another the ordinary addition table, while the third makes each addition by counting on his fingers, the three are presently out of sight of one another, whatever the rates at which the processes involved are performed. The lightning adder may proceed more leisurely than either of the others. He steps a league while they are bustling over furlongs or inches.

Now, the ability to take league steps in receiving telegraphic messages, in reading, in addition, in mathematical reasoning and in many other fields, plainly depends upon the acquisition of league-stepping habits. No possible proficiency and rapidity in elementary processes will serve. The learner must come to do with one stroke of attention what now requires half a dozen, and presently, in one still more inclusive stroke, what now requires thirty-six. He must systematize the work to be done and must acquire a system of automatic habits corresponding to the system of tasks. ✓ When he has done this he is master of the situation in his field. He can, if he chooses, deal accurately with minute details. He can swiftly overlook great areas with an accurate sense of what the details involved amount to—indeed, with far greater justice to details than is possible for one who knows nothing else. Finally, his whole array of habits is swiftly obedient to serve in the solution of new problems. Automatism is not genius, but it is the hands and feet of genius. ✓

COMMUNICATIONS FROM THE PSYCHOLOGICAL
LABORATORY OF HARVARD UNIVERSITY.

AUTOMATIC REACTIONS.

BY DR. LEON M. SOLOMONS,
University of Wisconsin.

The experiments upon the time of automatic reactions, of which I wish to give a brief account here, are an outgrowth in part of the work on Motor Automatism published by Miss Stein and myself in the REVIEW for September, 1896. I had three main objects—to see whether the various stages of automatism which we there distinguished had characteristic reaction times; to get evidence, if possible, for the theory advanced in that article, that the feeling of personal agency accompanying a movement is due primarily to the motor neurons of the cortex—that is, that it is the absence of their activity which gives a movement its feeling of impersonality; and third, to attack the problem of the relation of attention to the different types of reaction by studying reactions in which attention was totally absent.

The experiments are not complete, and their evidence is not as clear and convincing as it might, I believe, be made. But since it is doubtful whether I shall be able to continue them in the near future, and especially since some of the indications may prove valuable suggestions to other workers in the field, I think it advisable to give at least a preliminary account now.

GENERAL METHOD. The mode of distraction adopted was the same as in the experiments on motor automatism—the reading of light, entertaining literature. The stimulus was the sound of an electric hammer. During part of the experiments the Scripture reaction key was used. During the last part this was changed, since some of the subjects found difficulty in

maintaining the contact between reactions without interfering with the complete automatism of the movement. I accordingly changed to an Ewald key, but used a contact through mercury instead of the simple metallic contact. With this key a considerable unconscious pressure might be exerted by the subject upon the key without breaking the connection, and yet the reaction require no special effort. The mercury contact had only a very slight immersion—never more than $\frac{1}{32}$ of an inch—and did not, I believe, appreciably affect the reaction time, while it was of considerable assistance in maintaining connections during the intervals between the reactions.

The chronoscope—placed in a separate room to prevent the subject knowing when an observation was to be made—was connected in the usual way, the stimulus closing the circuit, and the breaking of the contact by the reaction opening it. Finding it difficult to maintain an adjustment of the fall hammer constant over long periods of time, recourse was had to a pendulum control. This had the disadvantage that the time of the control was greater than that of the reactions studied. But as relative values only were desired, this was no real difficulty, while the greater certainty of constancy of conditions from month to month was a distinct gain.

The subject was instructed to keep his attention as closely as possible upon what he was reading, and not to think of the experiment. He was asked to introspect as carefully as circumstances permitted, but not so as to interfere with the automatism. The subjects differed considerably in the ease with which they acquired the ability to react automatically, but the stages seemed to be the same in all.

At first the attention is all on the experiment, the subject reading without understanding. Gradually the incidence of attention shifts, and he is able to keep his mind on his reading between reactions, but has to stop reading to react. The interference produced by this reaction becomes less and less, until the various stages of automatism are reached and passed through. Some subjects become automatic after very little practice; others require a good deal, and their results are more valuable for the light they throw on the passage from voluntary

to automatic reactions, than for the passage from simple automatic to subconsciousness.

At first the reaction times were studied by the usual method of taking the average, corrected if necessary by throwing out those with very large residuals. But during this process it was observed that the small residuals were not, as they should be, in the majority; but that often, on the contrary, there were a large number of large residuals of about the same value, with few, if any, small ones. This showed that the average was simply a mean between two reaction times of different value, and, therefore, thoroughly misleading. Accordingly I adopted the method of plotting the reactions, as one plots an error curve. The resulting curve is, of course, of the same form as would be obtained by plotting the residuals, the position of the Y axis alone being changed.

The curves so obtained did not in general assume the form of the theoretical error curve, but showed a grouping of the reactions about several points. It had been my intention to study the effect of frequency, intensity of stimulus, etc., on the reaction times, and I had arranged my apparatus with that end. But finding the problem complicated by the reactions being of mixed types, I thought it best to confine myself to my main problem.

Owing to the uncertainty of the last figure of a reaction time obtained in thousandths of a second, I plotted the curves, during the course of the experiments, for hundredths of a second only. Becoming satisfied, however, that this method failed to bring out some important features of the reactions, I commenced a more minute study, with various methods of plotting. A comparison of these results convinced me that the best method for these results was to let the ordinate corresponding to any time represent the number of reactions having a value within 2σ of that time. This gives a curve the main features of which may be seen at a glance, but which is, nevertheless, not misleadingly simple.

It will be seen in the following discussion that I do not place much reliance upon the lesser variations in the curves. They are probably important, but the chronoscope is too inaccurate an instrument to warrant reliance upon them.

THE TYPES OF REACTION.—My subjects, eight in number, may be divided into three groups. Group one, consisting of subjects G, B and D, required long practice before becoming thoroughly automatic. They tended toward the auditory type. That is, their thought is largely in sound terms, and their attention is readily attracted and held by sounds. The subject G sometimes distracted himself by thinking of music he had heard. Group two, consisting of subjects M, S, and De, were of the visual motor type. They could not recall sounds at all. Their imagery was all visual and motor. These subjects readily became automatic and passed through all the stages of automatism. Group three, consisting of subjects Ho and Ha, were intermediate. They were poor visualizers, but their motor and auditory memories were good. They occupied an intermediate position as regards automatism. They found it difficult to keep the attention from wandering to the experiment. Their automatism, while in general apparently very good, was easily disturbed. These two subjects experienced the most difficulty in maintaining the contact during the intervals between the reactions. Whether the correlation here appearing between the types of imagery and the tendency to automatism is accidental or significant, remains to be seen.

Fig. 1 presents a series of curves obtained from the subject G. Each curve, except the first, represents the results of reactions taken at one sitting. The abscissa gives the time of the reaction; the ordinate, the number of reactions having that time, or coming within 2σ of it. The curves are arranged in time order, beginning at the bottom, and illustrate the progress of automatism. The subject G did not in general react automatically. He found it difficult to keep his attention away from the experiment, and when he did the reactions were often voluntary. That is, he had to turn his attention to the experiment when the stimulus came in order to react. He eventually became fairly automatic, however. His imagery is auditory and visual.

A glance at the curves shows immediately this characteristic. There are a large number of comparatively quick reactions in the earlier ones, then long reactions predominate, and then short

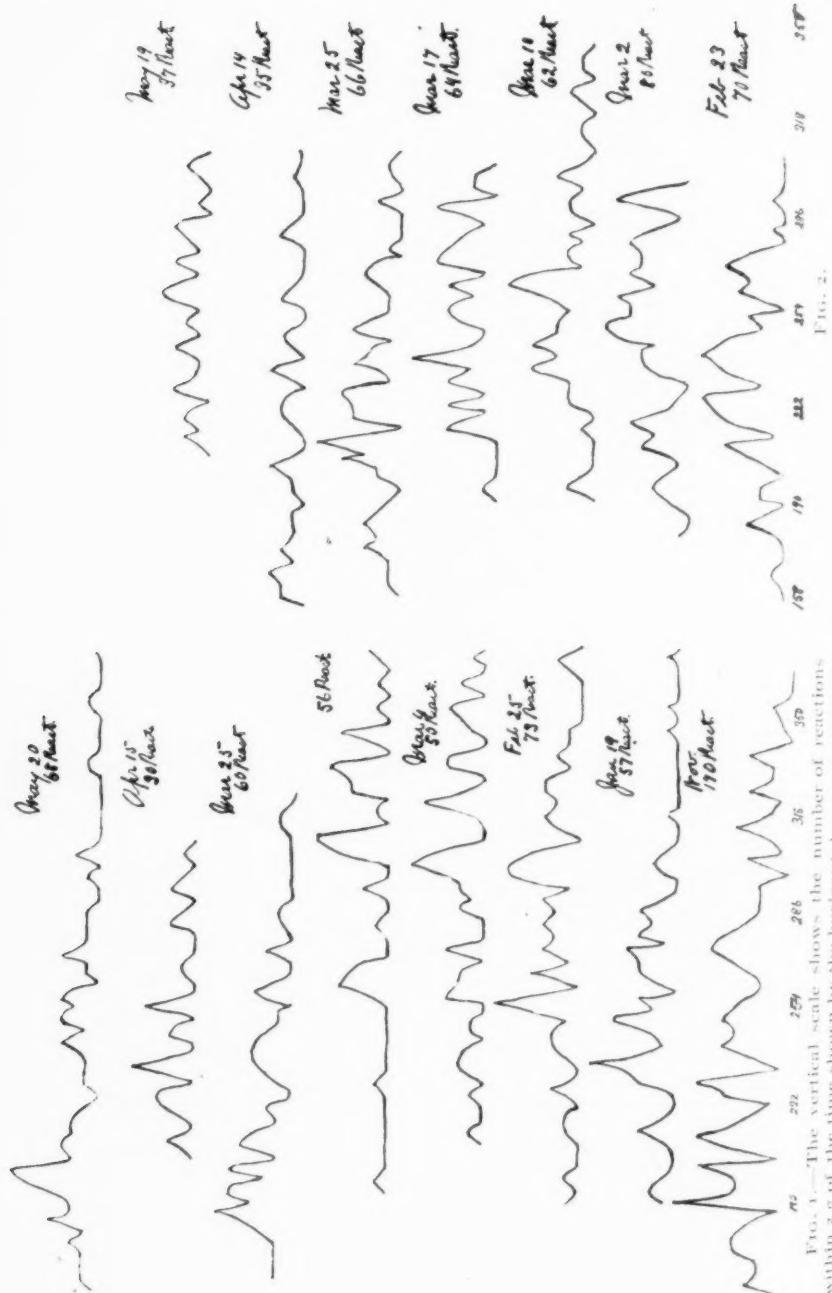
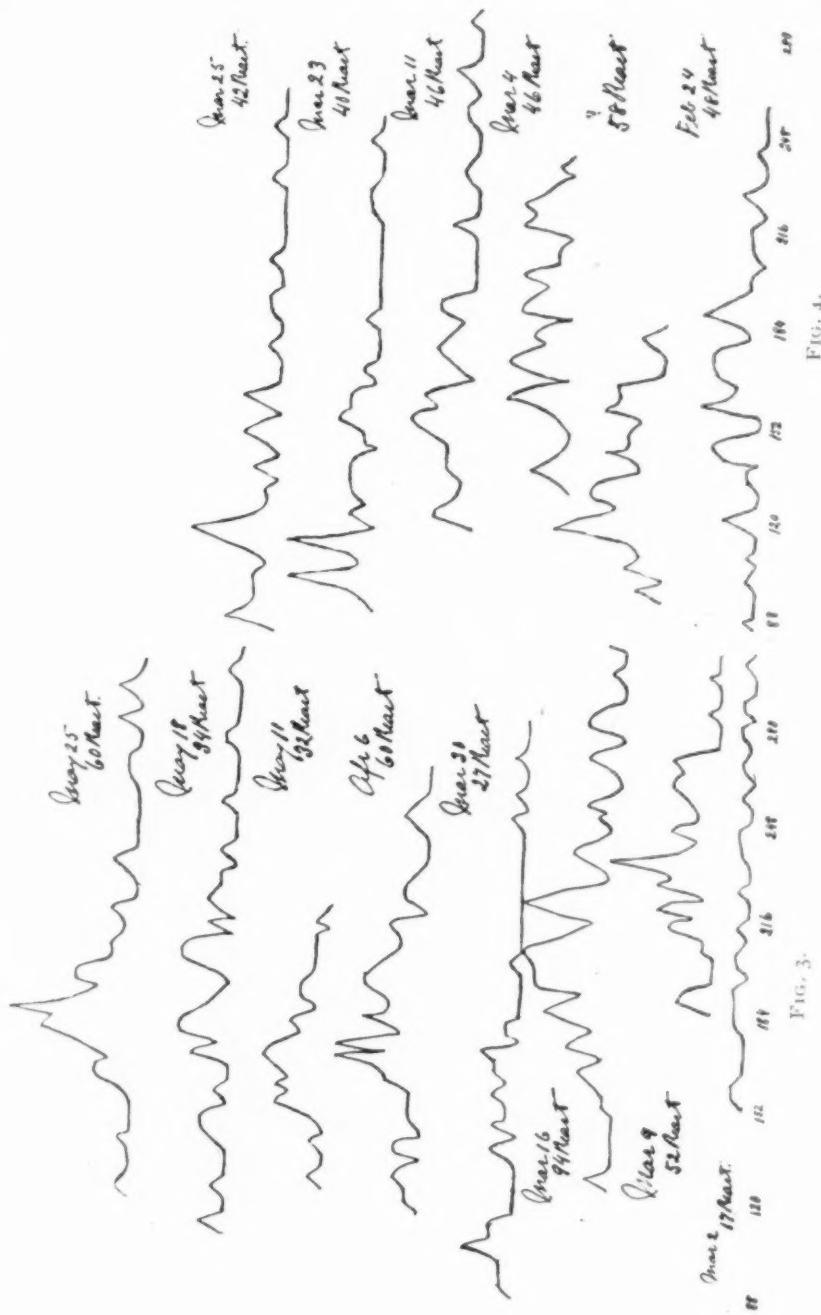


FIG. 1.—The vertical scale shows the number of reflections within 2 m. of the time shown by the horizontal scale.



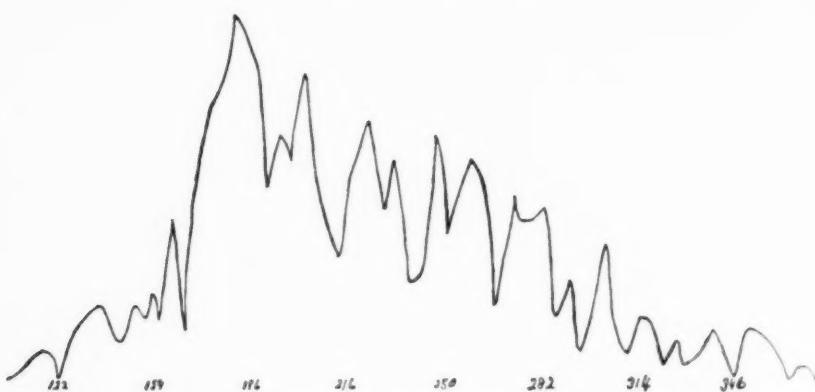


FIG. 5.—This curve shows the distribution of 518 reactions taken from 5 different subjects.

ones again. All the subjects showed this peculiarity. The reason is that at first the attention is on the experiment, and the subject in a condition of expectation, and we therefore have something near the conditions of the ordinary simple reaction. Later he learns to keep his attention off the experiment, and the reactions are slow. Then the path gets worn smooth by habit, and the various stages of automatism commence, ending in a very quick reaction.

The subject's notes amply confirm this explanation, if confirmation is necessary. G notes for the first curve, which represents the results of three days' observations during November, that his attention was more or less on the experiment all the time.

For Jan. 19 we have the note, "Attention somewhat on experiment, but not enough to give any really voluntary reactions. No very fast ones, as when attention is on reaction; nor any very slow ones, as when I do not react and then recollect myself." The results of self-observation are amply confirmed by the curve.

Feb. 25 the reactions were judged to be about 'in between voluntary and automatic.' On Mar. 25 the automatism is considered fair, and on Apr. 15 'more automatic than usual.' On May 20 the automatism was judged to be very good, and he expressed a doubt as to whether he heard the stimulus distinctly before he felt the reaction.

It will be noticed that during the period between the reactions in which the attention was on the experiment, and the appearance of good automatism, the bulk of the reactions are above 290σ . After automatism sets in the bulk of the reactions are below 290σ , and on May 20, when for the first time a doubt appears as to there being a distinct interval between stimulus and reaction, there are a large number below 230σ . The results from other subjects show that these peculiarities are significant.

Fig. 2 shows a similar series of curves from the subject D. Like G, the usual imagery of D is auditory and visual. She became automatic much more quickly, however, though remaining for a long time in the first stage. The first move represents the result of the first day's experimenting. She had very little difficulty in keeping her attention off the experiment, and after a very little practice the reactions ceased to disturb her reading. Nevertheless, it will be noticed that even in her case we have a greater preponderance of short reactions in the first curve. The next curve shown is for Mar. 2. Her report was 'Attention first attracted by sound, reaction automatic.' The next curve shown is for March 10. The number of reactions below 230σ is now at a minimum. She reported the reactions as seeming 'perfectly regular and automatic.' She always heard the stimulus first, and then the reaction followed, without an interval between, or any movement of attention, or effort. March 17 she was asked to compare the interval between the sound and the reaction, with that between the reaction and the click made by the key on striking. She found it difficult, but thought the second interval rather longer.

On March 25 I began giving the stimuli more frequently—every $7\frac{1}{2}$ seconds on an average, instead of every 15. The subject's judgment was that the greater frequency increased the automatism. In one sense this is apparently true. It should be noticed though that in her case, as in that of G, the introduction of the more frequent stimuli is marked by an increase in the number of short reactions greater than that of subsequent dates. I am inclined to believe, therefore, though they did not notice it themselves, that the greater frequency at first had the effect of

drawing their attention to the experiment a little, and that this effect passed away later.

On April 14 she notes that in one place the reaction and the stimulus seemed simultaneous, and that some of the reactions seemed 'impersonal.' It will be noticed that corresponding to this note we have a large number of reactions below 230, and several in fact below 180. Impersonality was never noted by her again, though on May 25 she again observed some reactions in which stimulus and reaction seemed 'almost simultaneous.'

It is to be noticed that D's reactions were nearly always below 290. The long period in which the reactions were above this, shown by G, is absent in her case, owing apparently, to the almost immediate occurrence of automatism. The subject B, the third of this type, gave results similar to G. She was a long time in becoming automatic according to her report, and her reactions showed a majority above 290σ for a long time. With D as with G, further, 'simultaneous' reactions were noted with the reappearance in number of reactions below 230.

The indications from these three subjects are, then, that the reaction time for automatic response to sounds begins somewhere in the neighborhood of 290. A reaction time longer than this indicates that some effort of attention or will is necessary. There is no change in this subjective condition until we reach a region below 230σ , when apparently a new type of automatic reaction begins. To study this other type we must turn to the records from other subjects. The exact limits of the first type, as well as the significance of the different groups of reactions indicated by the curve within this general type, had best be considered later.

As to the character of this group of subjects, supposing that it does represent a type of person, there is not, I think, any good reason for thinking the difference between them and others other than one of degree. With time and proper methods they will, I believe, pass through all the phases of automatism. I made no special effort to hurry them, for I was more than willing that some of my subjects should remain in this phase, for its better study. Instead of trying to adapt the conditions of

the experiment to the habits of attention of the subject, I made them the same for all subjects. Naturally the differing habits of attention resulted in differing responses upon the part of the subjects. As the practice was infrequent—I had none of my subjects oftener than twice a week—these individual differences had free scope.

Group 2, Fig. 3 shows a series of curves obtained from M. M is of the visual motor type. The first curve, March 2d, shows results of the first day. He reported no trouble in reading during the reaction, but an undercurrent of attention on experiment. His attention was attracted first by the stimulus. The stimulus and the reaction sometimes seemed simultaneous. The time between the stimulus and the reaction usually seemed shorter than that between the reaction and the second click.

On March 9 he reports his reactions rather regular. The stimulus comes distinctly first, then his feeling of reacting, then the sound marking the completion of the reacting. On March 16th, for the first time, some of the reactions seem impersonal. In these impersonal reactions the second interval, that between the feeling of reacting—a muscular feeling in arm or finger—and the sound made by the key, seemed *shorter* than the first interval. In a few reactions he can recognize the stimulus before the reaction, but in many he doubts whether he would know the order of events but for former experiences. On March 30th nearly all the reactions feel impersonal. The whole interval between the two sounds seems shorter, but the interval between the stimulus and the reaction feeling is about the same as that between the reaction and the second click. On April 6 the reactions are still impersonal. He gets the stimulus by a memory after-image. The reaction first attracts his attention, and then he is aware of the whole thing at once, though in the totality thus present the stimulus seems to be first. It seems to be a ‘succession of things all at once.’ In the latter part of the experiment the reaction was sometimes all over before he knew it, and the whole thing came as a sort of memory after-image. May 11, ‘sometimes the attention is first attracted by a funny feeling marking the completion of the reaction, a restless nervous feeling. On May 18 he reported a curious feeling which

was also noticed frequently by S. He knows the stimulus has come before he really hears it. It is a perfect imitation of an hysterical anaesthesia with 'clairvoyant' tendencies. The explanation is, presumably, that the sensory nerve current passes over into a reaction before rousing its usual response in the auditory centers of the cortex. The reactions on this occasion were only partly impersonal. They were frequently entirely over before he knew anything about it. On May 25th this characteristic was still more marked, being almost unconscious toward the end. The reactions were only in part impersonal. In some cases the stimulus and the reaction seemed all one; in other cases the reaction was almost simultaneous with the second click.

It will be noticed in this case that impersonal reactions do not appear until we have reactions below 180σ ; that they are not judged to be nearly all impersonal until the great majority are below this point; and that when this ceases to be the case the reactions are again only in part impersonal.

Further, it will be noticed that the first type of simple automatic reaction that predominated in the reactions of D and G—that is, a personal reaction with the stimulus coming distinctly and clearly first—is not noted after March 9th, when reactions above 230σ cease to be prominent. The indications then agree with those obtained from D and G. The first type of automatic reaction stops at about 231σ . The impersonal reactions begin below 180σ . How about the interval? The reactions between 180 and 230 are sometimes characterized by 'simultaneous reactions,' but not always. When they first occur they have this peculiarity. Afterward, though they are very distinct from both the impersonal and the simple automatic, they are difficult to describe. The subject M, it will be noted, only observed really simultaneous reactions once, though throughout the experiments he noted reactions not belonging to the other types. D, another subject of this group, only experimented once. Like M, he became automatic very quickly. He reported many 'simultaneous reactions.' The other subject belonging to this group, the writer, S, had a similar experience. I noticed simultaneous reactions very frequently at first—over a longer

period than M and De, but relatively short—but very seldom afterward. On two occasions this simultaneity was so marked and striking that I stopped the experiment to find out the record. Both showed reactions of 209 σ . In general it is not possible to get a judgment of the character of an isolated reaction—one can only get a general impression of a number. Nevertheless, though absolute simultaneity is not frequent, reactions which feel very similar to these are frequent and perfectly distinct. They are the quickest feeling reactions—unless we judge the time by the interval between the two clicks. They are characterized by uncertainty about the order of events, and a general predominance in the mass of feelings composing the total reaction—stimulus, movement, etc.—of the muscular and innervation feelings. Before passing to the general discussion of the results, however, it will be worth while to consider briefly the third group.

Fig. 4 gives a few curves from the subject Ho. Ho is a poor visualizer, but has a good auditory and motor memory. He was rather erratic in his reactions, sometimes being very automatic, and at other times not so. The first curve shown is for February 24th. By that time he had settled down to greater regularity. He notes that his attention is first attracted by the reaction. Also, that throughout the experiment there is a slight feeling of tension in the arm. For March 4th he notes some impersonality. March 11th, sometimes simultaneous, sometimes impersonal. On March 23d, "Not as automatic as usual condition of expectation. Some simultaneous, very few impersonal. Second interval most marked." On March 23d the more rapid stimuli—every 7½ seconds—were first introduced. With Ho, as with the others, their first introduction is marked by a preponderance of shorter reactions. But Ho notes a disturbance of the automatism, which the others did not. I believe the explanation is the same in all cases, but that only in Ho was the disturbance great enough to be noticed. In this case, whenever the shortness of the reaction is due to *attention* being on the experiment, the *short reactions do not feel impersonal*. Simultaneous reactions are, however, noted, though there are but two or three reactions within the interval where they usually occur. Both facts are, I believe, significant.

The last reactions are noted as very impersonal. The first interval seems the longer.

Perhaps the most important thing in these reactions is the indication of a fourth type of reaction below 130. The subjective conditions corresponding to these low reactions do not seem to differ much from those of the third type. They are, perhaps, a little more strikingly impersonal, and the second interval is still shorter. But these differences might appear in the third type after practice had worn its path smooth and the subject had grown more accustomed to its observation. I am inclined to think, therefore, that the difference between the paths indicated by these two groups of reactions does not involve any difference in consciousness; that the change is entirely in the lower centers.

GENERAL DISCUSSION.—Until the facts are more clearly established I do not feel justified in taking up the time of the readers of the REVIEW with a full discussion of their significance, for this would involve the presentation and examination of a much larger number of curves, a very tedious discussion, and, in the end, still much doubt and uncertainty. This would be worth while only if no more conclusive evidence could be obtained. But as I believe that more extensive experiments will save this, the proper course seems to be to give only a brief statement of the most general conclusions to which the experiments have led me.

Above 290σ we have reactions in which some element of will appears. In the slowest there is an idea of the movement about to be made. In those nearer to 300σ there seems to be no *idea* between the stimulus and the reaction—nothing but a feeling of voluntariness, of somehow willing what takes place. This is not the feeling of effort mentioned as one of the elements of a sensory motor reaction in my paper on ‘Normal Motor Automatism.’¹ The feeling of effort does not appear in these simple movements, unless the subject gets tired. It is rather a portion of what we called the ‘motor impulse,’ and described as “a *milange* of visual and kinæsthetic material, as well as other elements not easily described, and, perhaps, really a direct

¹ PSYCH. REVIEW, Vol. III., No. 5, p. 498.

consciousness of a motor current." The results of these reaction experiments permit, I think, a somewhat closer analysis of this motor impulse and the stages of its disappearance. The 'visual and kinaesthetic material' seems to disappear first, and then this peculiar will feeling. My chief evidence for this view is the statement of the subject G, on days when his reactions were largely between 280 and 340, that between the stimulus and the reaction there were 'feelings,' but no ideas or readily describable reactions.

Below 290σ we have nothing left of the motor impulse except the feeling of personal activity. In the typical reaction of this class the subject is resting quietly, when his attention is suddenly attracted by a sound—or, rather, he suddenly hears a sound, for there is no conscious movement of attention. Immediately after he *feels himself react*. Then he hears a click telling him that the key has been pressed down. During all this time he has gone on with his reading undisturbed. He is conscious of what has happened, but that is all. These reactions seem to correspond to the usual 'sensory reaction.'

The next type, from about 175 to about 225, is characterized by the prominence of the reaction feeling. When reactions of this type first appear their distinguishing feature is the simultaneity of the stimulus and the reaction. The subject's attention being fully on his reading, he is aware *at once* of a sound and a movement. He finds himself pressing a key at the same time that he hears a sound. Later he does not really hear the sound at the same time as he reacts. He is suddenly conscious of reacting, and later of two sounds. Of these sounds, the one seems to be a *memory after-image* of a sound made *before* the reaction, the other to be the *sensation* of a sound coming *after* the reaction. The explanation of this change seems to me to be this: In the first type the sensory current goes first to the auditory centers, where it awakens a response, and then to the centers, whatever they are, whose activity gives the reaction feeling, or the beginning of the reaction feeling, and then out to the muscles. In this second type the sensory current divides, part going direct to the reaction center, part to the auditory center, and rousing both to activity at about the same time. As

the new path gets worn less stimulus goes to the auditory centers, and they respond only after some time. To put it another way, with the establishment of the shorter path the attention gets more completely away from sounds. Now, whenever we fail to hear a sound immediately, and later turn our attention to it, we get it by a sort of memory after-image. This memory after-image has peculiarities of its own which enable us, or cause us, to apperceive it as such, and project it into its proper time relations, or what knowledge and habit would indicate to be its proper time relations. Thus, though the reaction is the first thing to come into consciousness, we apperceive the whole group of stimulus (perceived by memory after-image), reaction feeling and final click, according to previous experience and our knowledge of the particular circumstances. This view of the relation between the two types is in entire accord with the fact that subjects with active and sensitive auditory centers remain so much longer in the first stage than those whose motor centers are the more active.

In the third stage, the impersonal reaction, the last element of the motor impulse, has disappeared. In this type the reaction feeling is followed very quickly, if not accompanied, by the final click. Sometimes the subject heard the stimulus very distinctly before the reaction. Sometimes he is first conscious of the reaction, and gets the stimulus by a memory after-image;—but there is no doubt in his mind that the stimulus came before the reaction. What is the meaning of these observations? What has happened when the reaction becomes impersonal? The shorter interval between the reaction feeling and the final click, as well as the longer interval between the stimulus and the reaction feeling, seem to demand one, and only one, explanation. In the previous types the beginning of the reaction feeling was an activity in the cortex. In this the reaction feeling is purely a sensation from the muscles of the hand and arm. The sensory current must now go over into a motor reaction through the lower centers entirely, or, at any rate, without awakening any response upon the part of the cortex. To this extent then I believe that the theory advanced by Miss Stein and myself as to the origin of the feeling of personality is fully confirmed by

these experiments. The reaction becomes impersonal when the last center that contributes anything to consciousness drops out of the sensory motor path, and this center contributes nothing but this feeling of personality.

When we come to inquire more carefully into the identity of this center difficulties arise. The reaction feeling is the same in the impersonal and the personal reactions. It has changed nothing but its orientation, so to speak. It is felt in a different relation to the personality and the stimulus. The sensations are the same. How is it then that in the personal reactions the whole reaction feeling is timed by the part of it which simply gives its personal coloring? This fact suggests the view that this last center, which gives the personal relation, is a kinaesthetic center, and includes a feeling of the reaction identical with that furnished by return sensations alone. But this view in turn has, it seems to me, grave difficulties. All the kinaesthetic part of the sensory motor path seemed to have dropped out before the first stage of automatism. Moreover, in the personal reaction one is not conscious of both the reaction feeling and the return sensations. It is necessary, therefore, to suppose that the two fuse, though occurring successively. But if we admit that nervous disturbances separated by such an interval of time may fuse into one presentation, the necessity for supposing the center giving the personal feeling to be kinaesthetic ceases. The most natural supposition, then, seems to be that it is a motor center: and that its activity gives the personal feeling to the sensations that follow. I do not mean that the activity of the motor centers gives a consciousness of personality alone. The feeling that one has reacted is not a feeling of personal activity plus a muscular feeling. It should rather be said that *when the sensations from an arm movement are preceded by a discharge of the corresponding motor cells of the cortex they are felt to be personal.* The activity of the motor cells is thus responsible for the resulting state of consciousness taking this form. The impersonality of the reaction, or its personality, as the case may be, is not part of the reaction feeling, but a peculiarity of the whole state of consciousness in which the reaction feeling is represented in all its relations to the stimulus and the second click,

and to the reaction. It is this characteristic of the whole state of consciousness that is determined by the presence or absence of the activity of the motor cells.

As to the fourth group of reactions, if it exists, it must correspond to a still shorter path. The neuron whose dropping out marks the difference between this group and the preceding apparently furnishes nothing to consciousness, and is presumably outside the cortex. On the other hand, though, should it be thought that the feeling of personality comes from a kinæsthetic center, and that this is anatomically distinct from the motor zones of the cortex, the way is open to regard the fourth type as the first purely 'extra-cortical.' In the present state of our knowledge of the finer anatomy of this sensory motor path and the meagerness of these experiments it would be unprofitable to discuss further the correlation of the different types of reaction with known sensori-motor paths.

As to the third question, the relation of attention to reaction time, these experiments show that all types of reaction are possible without the attention being on any part of the reaction—in so far, that is, as we take the length of a reaction as an index of its type. They further indicate that the will has nothing to do with the ordinary reaction, its function being confined, after a little practice, to placing the sensori-motor path in a condition favorable to rapid reaction. The muscular reaction is practically a reflex—as the Leipsic school contend—and the sensory reaction is at least automatic.

Professor Angell's¹ view that the ultimate effect of practice is to reduce both types of reaction to the same time, seems to me to be confirmed by these experiments. Professor Baldwin's view, that the subject's habits of attention, as reflected in his usual imagery, is an important factor in determining his behavior in reaction experiments, seems also to be in accord, though my experiments do not throw any light on the more specific suggestions made by him as to the exact way in which these habits influence the simple reaction.²

My observations on the earlier reactions, when the subject's

¹ PSYCHOLOGICAL REVIEW, May, 1896.

² PSYCHOLOGICAL REVIEW, 1895, p. 259.

attention was still in part on the experiment, would lead me to believe that the principal effect of attention in this case is to bring the entire motor mechanism into a condition of heightened sensitivity. As a result, when the stimulus comes, all the paths, or many of them, are used. The reaction time is, of course, the time of the fastest; but the current also traverses the others. On this account the reactions never feel impersonal, but do very often feel 'simultaneous.' The motor cells always respond before the return sensations from the reflex reaction have arrived, and give the reaction a personal feeling, even though, in fact, it is reflex. But the division of the current between the paths of the first and the second type is the most favorable condition for 'simultaneity.'

Before closing, a few words may be said concerning the smaller groupings shown by the curves. Though in the curve representing a single day's reactions it is to be expected that some of these groups are mere matters of chance, this explanation will not hold for large numbers of reactions. In fact, a glance at the curves will show a great deal of uniformity in this respect, showing that even as few as thirty or forty reactions will give reliable groupings. Especially is the location of certain of the minima very constant from day to day. Apparently the changes in reaction time due to practice, and even the differences between one individual and another, are due primarily, if not wholly, to the relative preponderance of different groups, rather than to change in the time corresponding to the same group.

Fig. 5 shows a curve obtained from the reactions of five different subjects, during two weeks in May. I select this period because both subjects and apparatus were fairly constant in their behavior throughout it. It will be seen that the groupings are by no means destroyed by this combination of the results from several subjects and on several different occasions. More heterogeneous selections of results also continue to show the grouping in a very marked manner, but not so satisfactorily as this.

It will be noticed that much of the grouping shows a large group separated from its neighbors by deep minima, which is divided in turn into two groups, separated by a much slighter

minimum. This smaller grouping I do not consider reliable, as it may be largely due to the chronoscope. The larger groupings can hardly be so explained, and since they are not marked by differences in consciousness they presumably represent differences in the sensori-motor path outside of the cortex. The detailed discussion of this subject, however, I reserve until I can present fuller and more exact results.

In concluding, I wish to express my thanks to Professor Münsterberg and to my fellow-students in the Harvard Laboratory, for cordial coöperation and assistance.

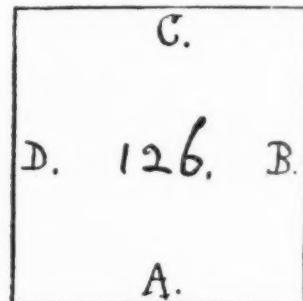
RECOGNITION UNDER OBJECTIVE REVERSAL.

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This research was carried on in the Harvard Psychological Laboratory during the first five months of 1898. It was undertaken for the purpose of determining the facts as to the relative ease of recognizing objects when seen a second time, but under various degrees and modes of turning or reversal in a plane at right-angles to the line of sight. Knowledge of these conditions has value and interest to psychologists on more than one account, for the problem of recognition is inter-related with the whole theory of space perception and with that of vision in general, while the curious relations in which right and left are apprehended by the subject are herein also implicated. The comparative ease and accuracy of the recognition of objects appear to be the sole criterion by which the relative naturalness of seeing, so to say, may be reduced to figures and so to scientific exactness, for we are all so fully accustomed to seeing things in any possible mode or degree of reversal, both objective and subjective, that comparison with a normal position in each case, with judgments by various subjects and in very numerous cases, is the only practicable means to reliable information on the subject. But in order that such recognitions may approach the threshold difficulty many objects quite unfamiliar to the subjects must be employed, yet objects sufficiently like each other to allow of reasonable comparison. These conditions, of a very large number of unique and unfamiliar objects easily made and handled, and comparable in all respects, are well satisfied in the choice made of the essential apparatus of this research, namely, the blots of ink, whose usefulness in Psychology was suggested by the writer in the REVIEW for May, 1897, p. 390, and illustrated and employed in a research into imaginations in the

American Journal of Psychology for January, 1898 (Vol. IX., No. 2, p. 183). In the present case these blots were made, each unique, to the number of about four hundred, on bits of white paper 4 cm. square and each attached to a card of thick pasteboard of equal size and shape; these were kept, arranged in order, in long, closely-fitting metallic cases. The blot-cards were numbered consecutively by figures on the backs, while the four edges of the card were lettered respectively A, B, C, and D, the arbitrarily chosen normal position of the blot or character being that in which A was at the bottom or, when lying flat, nearest the subject; similarly for the other letters, each represented a quadrant of reversal from the norm. The front and back of blot 126 are here reproduced, actual size, as an example.



Besides this regular series of blot-objects, each quite unique, there were employed twenty-one pairs of blots in which the components differed only in that each was to its mate as the right hand to the left, the so-called mirror-reversal. These were numbered and lettered similarly to the rest, with an R preceding the number. These ink-blots, thus prepared and marked for exact determination at all times and in whatever position, constituted really the simple apparatus of the experiments.

The schedules by which the characters were arranged invariably in series and sets of series, and in that order successively exhibited and judged upon, were made out by their numbers alone—that is, without any regard whatever to the

form or other quality of the blots themselves. They thus being taken quite at random in preparing their order, there was no possibility (because of their large number) that any differences that might exist in their suggestibility, ease of being remembered, similarity of successive characters, etc., should obtain, and so vitiate their proper recognizability; for 'chance' is in such a case, in the long run, a better safeguard than any deliberate selection by an individual could be.

To secure the requisite precision and a record of the times of observation required for each judgment, a simple and ordinary electrical apparatus was arranged, which may be described as follows: On the subject's table convenient to his left hand were fixed two keys, each actuating a pen tracing on a slowly revolving smoked drum, the left line indicating always the 'yes' judgments and the right the 'no' judgments. Between these record-lines, an inch or less apart, a time-line marking seconds was traced by a similar pen worked by a Lough electric pendulum. Thus in small space there was kept a complete record of every judgment, both as to its quality and as to the exact time its judging required. Subjective notes were also regularly written with the other records on the drum. Convenient to the right-hand of the subject, and piled face-up in a frame made to fit, the various blot-cards were successively exposed by the subject and judged upon.

TABLE I.

SET V.

Series 41	Series I. 42	Series 43	Series 44	Series 45	Series 46	Series 47	Series 48	Series 49	Series 50
281	R166	R166 ¹	299	306	316	320	327	334	341
282	282C	292	300	307	317	321	321B	335	342
283	288	293	293D	308	308C	322	328	328C	343
275B	R167	R167 ¹	301	309	318	323	323B	336	336D
276B	289	294	294D	310	R169	R169 ¹	329	337	344
284	284C	295	302	311	319	324	330	330C	345
285	R168	R168 ¹	303	312	R170	R170 ¹	331	338	338D
286	290	296	304	313	313C	325	325B	339	346
279B	291	297	305	314	314C	326	332	332C	347
287	287C	298	298D	315	R351	R351 ¹	333	340	340D

The 368 blots employed were arranged in successive series of ten, with ten such series in a set, one set being the number

judged upon at each day's sitting. The plan being to expose to the subject certain of the blots twice, or sometimes thrice, in various quadrants of reversal, the five sets were arranged in a manner best conveyed by the representation of an actual set-scheme here. Cf. Set V., reproduced in Table I. This represents the invariable order in which the series of blots were presented to the subject, always with an accurate interval of *three minutes* between the beginnings of the successive series. Thus the time which elapsed between perception of the character in its normal position and the judgment as to recognition was in each case nearly constant. It will be observed that, with the exception of Series 45, in which all the blots are new to the subject as a test, 30 per cent. of the object-figures are repetitions, a fact, of course, quite unknown to the subject, as were all other details of the schedule. In this set Series 41 will be seen to have its quota of repetitions like the rest, but judgments upon the three have only secondary interest, and are not counted, because seen perhaps days, instead of the regular three minutes, before. In Series 42, then, it is evident that the second, sixth, and tenth characters were repeated and quite inverted, as the *C* in each case indicates. In Series 43 the first, fourth, and seventh blots were repetitions; but here the normal (*A*) positions of the mirror-reversal were compared, as also is the case in Series 47. In Series 44 and 50 the degree of reversal was 270° , or three-fourths (*D*); and in Series 48, 90° , or the *B* reversal. By such a degree of irregularity in placing the repetitions in the series all chance of suggesting any regularity to the subject was avoided.

The instructions given to the subject were as follows: "Make your judgments yes or no in answer simply to the explicit question, Have you ever seen this blot before? Make your judgments only when a feeling of *certainty* is in consciousness equal to that attained from the comparison of two blot-characters, one seen just before and the other never seen before. When ready, make your judgment reactions with equal energy and promptness in all cases, so that the mere time-reactions, as a constant function, may be disregarded. Let a quick double reaction on a key indicate extreme certainty, and a prolonged pressure a

proportional degree of doubt. Read or converse between series, so as not to review in imagination the blots just seen. Report all possible subjective notes of interest." Thus the subject knew nothing of any reversals, of the number of repetitions in each series, nor of the purpose of the research even; he or she merely answered yes or no to the question as to recognition, when 'certain' of his or her judgment in that regard. Within the period of three minutes no incentive to hurry was suggested.

TABLE II.

		REPEATED BLOTS.					
SUBJECT.	REVERSAL POSITION.	A.	B.	C.	D.	R ¹ A.	R ¹ C.
1.	Recognitions. Per cent.	61	30	51	16	54	57
	Whole No. cases.	33	33	97	19	37	14
2.	Recognitions. Per cent.	33	24	39	15	45	25
	Whole No. cases.	12	21	61	14	22	4
3.	Recognitions. Per cent.	91	73	76	60	73	40
	Whole No. cases.	12	15	41	10	15	5
4.	Recognitions. Per cent.	62	25	29		40	28
	Whole No. cases.	21	12	35	1	10	11
5.	Recognitions. Per cent.	91	17	53	75	55	40
	Whole No. cases.	12	12	32	4	9	5
6.	Recognitions. Per cent.	55	33	43		50	0
	Whole No. cases.	9	6	23	1	4	5
7.	Recognitions. Per cent.	70	67	52		50	33
	Whole No. cases.	10	6	22	1	4	6
8.	Recognitions. Per cent.	0	17	33	0	0	
	Whole No. cases	3	6	9	3	6	0
9.	Recognitions. Per cent.	67	100	83			
	Whole No. cases.	3	3	17	1	1	2
Average per cent.		70	43	51	33	46	32

The seventy blot-cards required for one sitting being, then, arranged in order on a table behind the subject seated at the apparatus, the method of procedure was, in brief, constantly as follows: The time-pendulum being in action, the first series of

ten blots was arranged according to the scheme for that set (as exemplified above) and placed in the holder face up, but covered, before the subject, whose left hand covered the judgment-keys. At a given signal, whose exact temporal position was carefully marked on the time-line, the subject lifted the blank covering card and exposed the first blot, reacted yes or no in the proper manner when his judgment was made, turned the used blot-card face-downward near by (so that marginal retinal images would not interfere), then immediately proceeded to the next, and so on through the series of ten. After an interval of three minutes from the time of beginning on the first series the second was begun, and so on through the set for the day. This number of judgments, although occupying only thirty minutes, was found quite sufficient for the best work of the subjects, they generally reporting the judgments very tiring, especially those who are 'motiles' or 'audiles' in imaginative type.

Nine subjects were employed in these experiments; two of these were professors of philosophy, and the rest students and instructors in the Laboratory, one being a student of Radcliffe College.

The figures which represent the more interesting part of the results of the research are given in the accompanying Table II., useful as a matter of record chiefly. Altogether they represent over 2800 judgments (and none too large a number); of these, 30 per cent. are on repetitions. In the tables, A indicates

TABLE III.

SUBJECT.	UNREPEATED BLOTS.			
	"RECOGNITIONS." (FALSE.)		NON-RECOGNITIONS. (TRUE.)	
	PER CENT.	CASES.	PER CENT.	CASES.
1.	21	126	79	466
2.	19.5	62	80.5	256
3.	51	133	49	126
4.	22	61	78	215
5.	27	52	73	144
6.	27	35	33	96
7.	24	32	76	101
8.	16	10	84	54
9.	65.5	40	34.5	21
Avg.	30.		70.	

normal position of blot; B, 90° reversal over toward the left; C, inversion; D, 270° reversal over toward the left; R'A, erect mirror-reversal; and R'C, inverted mirror-reversal. The last tabulation of figures (Table III.) gives the numerical details of the judgments on the blots exhibited to the subject but once, but judged as to recognition in the same manner as the rest. Of this class there were somewhat over 2000 judgments, or 70% of the whole number of exposures. It will be noticed from this second table of result figures that seven of the nine subjects judged that they recognized approximately one out of every five of the blots which in reality they had not seen before, about 22% of their so-called 'recognitions' of these unrepeated blots being mistaken. Of the two remaining subjects, one (number 3), with 259 of this class of judgments, 'recognized' over 51%, and the other (number 9), who, by the way, reported especially 'certainty in most cases,' thought that he recognized 65.5% of characters which he had never before seen. On the average the percentage of false 'yes' judgments was 30%. The cause of a part of this error rate is evidently to be found in the actual formal similarity which some of these chance blots bear to each other, a circumstance not, however, to be eliminated from any set of objects of necessity so numerous; indeed, in these characters this similarity is reduced to a degree which lends continual interest to their use. Cases of great doubt were most often, and not unnaturally perhaps, put on the 'no' line of judgments, a 'doubtful' key and record-line having been for a time employed, but of necessity soon abandoned, because through its over-use the research threatened to be vitiated.

Having now before us more or less complete the data of the experiments, let us try to make more plain the circumstances to which they relate.

The objective conditions of the research are obviously the simplest which are logically possible for a comprehensive study of the natures of reversal and of recognition, the mental confusion of memory blot-images, arising from the large number seen, serving only to reduce the stimulus to the threshold-intensity—a requisite of the method here employed. Owing, however, to this confusion, in the main the 'feeling of recogni-

tion' was by no means regularly present in any degree, and often not at all, recognition usually taking place by the suggested association in the reviewing consciousness of some cognitional fact that had formed part of the perception or feeling present when the blot was first seen as the norm. Most often, indeed, it was some precise fact, remembered in terms of number, or some extrinsic suggested resemblance, or even by some wholly external complication whose relation could not perhaps be traced. Very often the recognition depended on the recall of some very small portion of the blot, such as a peculiar point or knob, or some more than usually grotesque end or corner. Though small, the characters were so rich in detail that often the whole as such was not in any proper sense perceived. Recognition, however, most often depended on apperception, and not on any feeling of recognition, and could be classed as of the mediate, rather than the immediate or general variety.

Some of the subjective notes as to the various methods of remembering and recalling may in this connection have some interest, although it was not easy to throw light on a process which required only the brief times which the subjects usually deemed sufficient for a 'certain' judgment of a blot. Four, then, of the subjects reported that the characters instantly called up actual objects by association, and that their recognition occurred by this means. Two subjects, on the other hand, reported that such products of imagination did not appear in their cases. One subject (and he who made more judgments than any of the others) reported a constant tendency for the blots immediately to place themselves in certain classes as regards general form of outline, proportional size of their various parts, mode of shading, etc. Most of the subjects remarked the immediacy of the judgment oftentimes (obviously often being wrong cases of auto-suggestion), and in cases of great doubt, that the ensuing confusion made matters worse and judgment sometimes vain. Two subjects reported their method to be to 'count the tails' or projections of the object. The two subjects who made the greatest number of wrong 'yes' judgments made likewise the greatest number of right 'yes' judgments, one of these two being he who reported 'great certainty.'

and the other a man known in his college for his self-asserted unmistakability. The subject who made the smallest average of recognitions was a man noted as an unusually partial 'motive,' who often reported it most difficult for him to recognize the blots at all, although he made more judgments than any of the other subjects save one. This subject, with two others, also reported that he never apperceived reversal as such, while to others it was regularly in consciousness when it occurred. Although the same set of blots was judged upon by the same subject in some cases several times on different days often a week or two apart, no improvement was visible in the record of such a set, and no suspicion ever entered the subject's mind that this repetition was occurring. This was true in one case where the subject saw the same set six times with no statable improvement. Owing to this circumstance hundreds of blot-cards sufficed where else thousands would have been required.

The time required for each of the more than twenty-eight hundred judgments was exactly recorded; but it has appeared that so great is the complexity of the conditions subjectively and objectively, especially as regards individual differences and as to temporary mood, that nothing of interest in this direction can be given in precise numerical terms. The reaction time for the mechanical process of uncovering a blot, pressing the proper key and overturning the blot-card when used, was, approximately on the average, one second. The total times vary then from almost this period to often eight or ten seconds; the average time required was not far from three seconds (and this when nearly as many minutes was the outer limit). Length of judgment time seems to have no constant relation to accuracy, owing evidently to the quickly-arising confusion on introspection; yet the most accurate judgments on the average were made by the subjects whose time records are the most irregular, as occasional retrospection on the occurrence of a doubt would necessitate. The inevitable effort which most subjects make to produce a rapid record, despite remonstrances, did much here, as elsewhere in psychological experiments, to reduce the accuracy of the judgments; but as long as human nature remains as it is it will be so.

The purpose of the research was to determine the relative recognizability of objects erect and in various modes and degrees of reversal. Let us examine the results in this regard, and try to suggest their meaning, psychological and physiological, as far as may be. Of the blots repeated or reviewed in the normal or A-position, recognitions were on the average 70%, and this almost exactly corroborates the average percentage of right judgments as to unrepeated objects, 70% being there also the nearest whole number—in other words, the average of recognitions of erect repetitions and the average of non-recognitions of unrepeated blots exactly prove each other. This 70% is, then, properly the standard of the research, with which the averages under the various reversals are to be compared. This is the proven general personal equation, so to say, of recognition of these objects when repeated exactly, without any objective complications. Using this, then, as the standard of 100% (A-position, or normal), B-reversal, or a quarter-turn (in a direction opposite to the hands of a watch), gives 61.4% of recognitions; the C-reversal, or complete inversion, 72.8% recognitions; the D-reversal, or a turning of 270° , 47.1%; the R'A reversal, or erect mirror-position, 65.7%; and the R'C reversal, or inverted mirror-position, 45.7% of recognitions. In other words, it appears from the research (and this is the kernel of its interest) that *an object is recognized more readily when inverted than in either of the two intermediate portions of quarter-reversal, and more readily than in the erect mirror-position or that position inverted*—an object upside down appears more natural than when turned on its side or seen in a mirror. Furthermore, one-quarter reversal toward the left is more favorable to recognition than three-quarter reversal (important only for certain forms); while least favorable of the six positions compared in these experiments is the inverted mirror-reversal, most rarely encountered of them all in general experience.

These facts are simple, while their explanation in psychological terms is neither so easy nor so sure. Yet something in that direction may be suggested here.

The great Law of Habit, individual and inherited, seems in general to furnish sufficient reason why uncomplicated repeti-

tion—that is, when the blot-object is repeated in the normal position—should be more easily recognized than in any case of several, this being the condition ordinarily in experience. Experiments in which the experimenter wore for some weeks before his eye a lens which inverted his field of vision have proven that it is comparatively easy to get accustomed to objective inversion even of objects seldom or never ordinarily seen so reversed. Indeed, to the lowest orders of animal life inversion must be the rule of their experience, to them making no difference. It is easy to conjecture that a sufficient degree of atavism in vision is easily brought about even in cases like those of the present research, making inversion relatively natural; furthermore, it must be remembered, that the retinal image is an inversion of the object, a fact adding little here, save of possibility. It would be interesting to know from experiments if a person unfamiliar with either condition could not learn to read print upside down more easily than print made from type turned on their sides. At any rate, the perfect facility with which printers read directly from the type in any position as ordinarily set shows how easily reading under inversion becomes natural.

Again, on the other hand, vision of objects turned on their sides (or one-quarter reversed) is very seldom experienced, indeed, and especially in the case of unfamiliar objects. Nothing in organic structure or in physiologic habit affords practice in this sort of recognition. The longer axis of many of these blots runs vertically or else horizontally, and from the fact that the right halves of both retinae are 'supplied' by only one visual center, the left, and *vice versa*, it is obvious that a quarter reversal of these objects would involve more new brain elements than would their inversion, and so make them seem less familiar, often very likely, in the former case, stimulating both cortical sight centers as not in the latter case; at any rate, implicating else unused 'apperceptive cells' or regions.

As regards the apparent difficulty of recognition under three-quarter reversal over that in the case of one-quarter left reversal, it is pertinent, perhaps, that our almost universal habit in reading is to begin at the upper left-hand corner of the page or card, thence looking along the top; and similarly more or less in per-

ceiving all plane representations. This firmly-fixed habit doubtless holds in the perception of these blot-characters, the spot receiving the focus of attention at first being undoubtedly in general the upper left-hand quadrant of the object, or at least so in some degree. A quarter-turn then would present for recognition a more or less familiar percept, while three-quarters reversal by the same principle would offer to the attention a wholly new portion of the blot, a portion, in fact, at the first instant of viewing, quite out of range of the habitual perceptive field, and so, from this circumstance, less fully apperceived.

With the mirror-reversal, finally, all are fully familiar from early and constant perception of the hands, feet and limbs in general, and from considerable experience with the use of mirrors, both natural and artificial. Here again habit affords relations which our research only exemplifies. Here, too (and more exactly), the easily acquired habit of reading directly from type is an instance and an illustration. Add inversion to the condition of common mirror-reversal, and the most complicated position of the six here studied is produced, a relation to the subject practically never experienced under ordinary conditions. Habit here has had no chance to produce an effect, and we have found that the percentage of recognitions is in this case the lowest of them all.

Further experiments should amplify these results, employing yet more involved relations between subject and object, varying them indefinitely. Especially would it be of interest to know if subjective reversal of various sorts would bring out the same results as to the habits of our seeing. Indeed, subjective reversal would seem to be a field fertile in many respects, both physiological and psychological.

SHORTER CONTRIBUTIONS AND DISCUSSIONS.

A LECTURE EXPERIMENT IN HALLUCINATIONS.

An experiment to illustrate a popular lecture must be striking, quick and sure to work. As it is not always easy to tell beforehand whether an experiment will answer these requirements, the following scheme for the production of a hallucination of smell may be worth recording. I had prepared a bottle filled with distilled water carefully wrapped in cotton and packed in a box. After some other experiments I stated that I wished to see how rapidly an odor would be diffused through the air, and requested that as soon as anyone perceived the odor he should raise his hand. I then unpacked the bottle in the front of the hall, poured the water over the cotton, holding my head away during the operation and started a stop-watch. While awaiting results I explained that I was quite sure that no one in the audience had ever smelled the chemical compound which I had poured out, and expressed the hope that, while they might find the odor strong and peculiar, it would not be too disagreeable to anyone. In fifteen seconds most of those in the front row had raised their hands, and in forty seconds the 'odor' had spread to the back of the hall, keeping a pretty regular 'wave front' as it passed on. About three-fourths of the audience claimed to perceive the smell, the obstinate minority including more men than the average of the whole. More would probably have succumbed to the suggestion, but at the end of a minute I was obliged to stop the experiment, for some on the front seats were being unpleasantly affected and were about to leave the room. No one in the audience seemed offended when it was explained that the real object of the experiment was the production of a hallucination.

Hallucinations of temperature or pain are easily induced by suggestion in susceptible individuals by the use of magnets, though the experiment is not suitable for lecture purposes. It is, of course, necessary that the subject should have hazy ideas about magnetism, but it is unfortunately only too easy to find such persons. The 'magnet' need not be magnetized, but should have plainly marked poles and the suggestion be conveyed by suitable 'patter', to use a conjurer's phrase. Sensations of heat may be produced by the north pole of the magnet,

and cold by the south, or one pole may be made to give a tingling or smarting pain in the right hand and side of the body, and the south pole on the left, or any other such scheme not too complicated. The illustrated magazine articles of the effects produced on hypnotized subjects by Luys, with magnets and sealed tubes of chemicals, are useful to reinforce the suggestions. Of course, the deception should be thoroughly explained after the experiment, not only because otherwise the subject sometimes complains of pain in the hand worked upon, but also in order that the experiment may serve as a lesson to the subject no less than to the spectators.

Slight hallucinations of sound are easily induced; but I have never succeeded in getting unhypnotized subjects to see red and blue flames on the poles of a magnet, or in obtaining any similar hallucinations of sight. Simple experiments in suggestion on persons in a normal state are generally better for demonstration than the more striking results obtained in hypnosis.

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PROFESSOR HYSLOP ON MYSTICISM.

In the last number of *THE PSYCHOLOGICAL REVIEW* Professor Hyslop criticises a paper on Psychology and Mysticism which I published in the *Atlantic Monthly*, and have since reprinted as the last essay of my recently published book ‘*Psychology and Life*.’ My paper was for him ‘one of the most amusing documents that he has ever had the pleasure of reading.’ I have not the slightest desire to disturb this happy mood of amusement by a serious defence against his attacks. A scientific defence or discussion must have as its aim that the opponent shall understand and agree with me; but I feel myself so absolutely free from this ambitious aim that a discussion is really superfluous. In regard to only one passage of my paper does he claim that he does understand what I wish to say and would agree with me; it is my reference to communication. “As to what Professor Muensterberg may intend by this description of the communication of ideas I can well imagine. But I can do it only by having some knowledge of the process myself, and not from any statement that he makes.” And then he goes on to interpret my meaning in a way which is, in every respect, the exact opposite of my thought, and which would deprive my arguments of all meaning. If he had not found anything in the paper which he believed himself to understand, I

should, perhaps, have taken the trouble to enter into a discussion that he might feel that he understood me. But after this test case I know that we think with a different logic, and I prefer that my statements continue to be for him 'blank nonsense.' I, therefore, do not argue, but wish merely to put straight a few facts which Professor Hyslop mentions as if he objectively reproduced my own words and statements, and where the reader of his criticism might believe that I am truly represented.

Professor Hyslop says: "His reason for not making a personal investigation into this question is that it is not 'dignified to visit such performances' as Séances ! ! " That is all, and what follows are merely exclamations of contempt for such an utterance. My text says this: "I consider it undignified to visit such performances as one attends a variety show for amusement only, without attempting to explain them." Is it really possible not to see the difference between my statement, with which every decent scientist will agree, and his false denunciation, which must make me contemptible to every scientific man?

Another illustration: "Professor Münsterberg did not distinguish between the relevancy of the various alleged phenomena that he was criticizing; table turning, telepathy, clairvoyance, hypnotism and what not were lumped together with no more conception of their differences than is usually displayed by the spiritualist himself." This is, indeed, very bad on my part; but the reader will become a little milder if he chances to take the trouble to open my article, and convince himself that more than half of the paper is expressly devoted to the clean discrimination of these and similar conceptions, and to the disentanglement of hypnotism from the rest.

A third illustration: I had said that up to the last summer vacation, in which I read systematically telepathic and spiritualistic literature: "I had not really studied all the recorded Phantasms of the Living and all the Proceedings of the Societies for Psychical Research, and I am afraid I had forgotten to cut the leaves of some of the occult magazines on my own shelves." Out of this material Professor Hyslop makes a confession, on my side, that until the last summer vacation I felt guilty of forming and stating opinions on this subject 'before reading its literature.' Because I have not read 'all the reports' and 'all the proceedings' I have not read the literature. If I really did not care to read the literature, why did I then subscribe for the occult magazines on my shelves? And immediately after it, Professor Hyslop says that he himself thinks that there are not twenty-five volumes in existence on this subject that any sane man ought to read

at all. As all the reports fill hundreds of volumes, he thus says clearly that a sane man ought not to read them all; and yet because I say that I had not read them 'all' he denounces me for confessing that I formed opinions 'before reading the literature.'

I do not care to go on; the other remarks are in the same spirit. Professor Hyslop says about me: "He thinks the scientist is trained to an instinctive confidence in his coöperators;" and he answers: "A man who cannot protect himself against fraud must not expect his opinion to be worth very much." I think both sides are correct here. I think, indeed, that a scientist is trained to an instinctive confidence in his coöperators, and I for one am inclined to consider in this sense even my critics as my coöperators, expecting that in spite of disagreement they will quote me correctly. But if the distortion transcends certain limits, I think Professor Hyslop is right in demanding that the scientist ought to discover it, and thus to protect himself in spite of his instinctive supposition that such things are impossible.

HUGO MÜNSTERBERG.

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PSYCHOLOGY AND LIFE.

The appearance of Professor Münsterberg's book with the above title calls attention afresh to the various points which have been criticised as the chapters have appeared in the *Atlantic Monthly* and elsewhere.

Professor Münsterberg refers to my former reply to one of his articles (not reprinted) as an unjust criticism, and one which leaves his opponent still unreconciled. I hasten to assure Professor Münsterberg that his subsequent article on Education, reprinted in this collection, fully grants all I ever thought of asking in that criticism, and much more.

I do not think it wise, at present, to try to teach experimental psychology in the high school. At the same time I wish to protest against the fundamental position of this whole book, in the hope that certain other points of difference may be as happily adjusted.

I am certainly in hearty sympathy with the great questions at stake. Professor Münsterberg is to be congratulated upon the whole-hearted way in which he emphasizes the realities of life, of the will, of feelings and their values, over against the mechanical, lifeless forms of which we see so much in current psychology. But what higher law makes it necessary to set these realities outside of psychology?

Who determines the limits of our science, and says we shall not include in it anything not fully explained by the law of causation? Who says the biologist may not stand face to face with the facts of life even while he is a scientist?

Undoubtedly the feelings of effort and strain connected with will acts are sensations; but, after taking them away, very much is left. Why, then, is it necessary to step outside of our text-book of psychology to say so, and to call upon the student to go to real life to see for himself what mind is?

The same is true of the emotions. The pleasures of expanding chest and relaxing muscles as one watches a sunset are sensations; but something remains after these are subtracted, and psychology has a right to call attention to the fact.

If necessary, in order to admit these subjects to psychology, I should be willing to question the fundamental propositions in Professor Münsterberg's epistemology which makes it necessary for him so to limit psychology. The world of things and the world of ideas are not two sides of the same thing in any sense in which the world of will and feeling is not also a side of the same reality.

All the history and traditions of American psychology call for a study of real life. That is what has given psychology the place it has always held in our colleges and universities; what gives it its firm position among the sciences to-day. Psychology is dealing with real life, and is able to make that life richer and fuller. Experimental psychology's recent development rests upon that very fact.

No sensationalism or mechanical theory of association will ever take the place of a Hopkins, a Porter, or a McCosh.

Professor Münsterberg's insistence upon the realities of the inner life is but one sign of a returning of psychologists to real life. Let us make our psychology broader and deeper, not give it up altogether, if this, his new view, prevails.

We might go back to the old name, mental philosophy; or we might adopt some new name, like ethology, which has been recently suggested. But the word psychology has proved its right to remain. In the minds of men at large it does stand for reality, and that is why the world is turning to psychologists for solution of the problems of life. We ought not to refuse to answer while we settle questions of terms or the imaginary limits of our science.

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A REPLY TO "THE NATURE OF ANIMAL INTELLIGENCE AND THE METHODS OF INVESTIGATING IT."¹

My first duty is to beg the reader's pardon for a certain personal tone in this discussion. As Professor Mills has mentioned Dr. Thorndike twenty-nine times in his article, this reply will of necessity contain the word 'I' oftener than one would wish.

There are two sorts of assertions in Professor Mills' article: first, a number of important objections to a certain method of studying animal psychology; second, a number of attacks on my 'Experimental Study of the Associative Processes in Animals.'² The former I am glad to have the opportunity to discuss, because they should be of real interest to all comparative psychologists. The latter can be safely left to the judgment of anyone who has read the monograph itself, and will be taken up here only because that monograph has probably been seen by only a few of the many who have read the attack upon it.

Let us turn first to the important objections to my method of studying the formation of associations in animals. I say my method, because it seems likely to be thought of chiefly in connection with my experiments, though Lubbock used practically the same method with insects. It is, in fact, odd that Lubbock's recommendation as to insects was not sooner followed with mammals. He says, "In order to test their intelligence, it has always seemed to me that there was no better way than to ascertain some object which they would clearly desire, and then to interpose some obstacle which a little ingenuity would enable them to overcome" (*Ants, Bees and Wasps*, N. Y., 1896, p. 247). He used food as the 'object,' as I did, and interposed mechanical obstacles as I did.

Professor Mills' weightiest objection is that, when confined while hungry in such boxes and pens as I used, the dogs and cats were in a 'panic-stricken' condition and, therefore, temporarily lost their normal wits. Now, it is true that in many of the trials with cats and chicks, notably the first ten or twenty trials with each animal, there is often, as I fully noted, great violence and fury of activity. And this *might* be the result of mental panic, and so might be a sign of a loss of normal mentality. But the animals (the dogs and some of the cats) which did not display this excitement and fury did not display any variation in the results toward more intelligence. Nor did the animals

¹ By Professor Wesley Mills, pp. 262-274 of the May number of *THE PSYCHOLOGICAL REVIEW*.

² *Animal Intelligence*, Monograph Supplement, No. VIII., to *THE PSYCHOLOGICAL REVIEW*.

which showed certain results in the experiments of which confinement in small boxes was an essential feature show any variation from those results in the experiments (see pp. 87-91 and 96 of the monograph already cited) in which there was no excitement, no different activity from that shown all the time. In these experiments the cats were in the big cage which had been their home for weeks.

Furthermore, it seems unlikely that in the case of the animals which had already been the subjects of two or three experiments, and which had been in such boxes a hundred or more times, the violence and fury of activity could have been the result of fear or in any way a sign of its presence. For, as was stated in the monograph, such animals which have been made during a number of trials to crawl into these boxes which Professor Mills supposes were so disturbing to them, *habitually of their own accord went into them again and again*. Nor did they try to escape when I picked them up to drop them in. In the experiments in which I moved the animal's limbs, putting him through the movements, there was after from 0 to 12 trials no fear of my handling. (See p. 68 of the monograph.)

In short, all evidences of panic may be absent without any change in mental functioning, and the only cause of mental panic which would seem probable, namely, *fear*, was certainly not present in the greater number of the experiments. So I feel bound still to maintain the account given in the monograph, and attribute the animal's fury of activity not to mental panic, but to a useful instinctive reaction to confinement. It should be remembered that even in the midst of the utmost activity the cats would take instant advantage of any chance to escape which appealed to their instinctive equipment (*e. g.*, the widening of an orifice). It should further be remembered that the most violent animals did the most pseudo-intelligent acts. If any one of the eight or ten psychologists and biologists who saw the experiments in progress had seen signs of mental panic in the animals I should have inserted this discussion in the monograph. But I venture to think that if Professor Mills had repeated five or six of my experiments he would have discarded this mental panic objection.

The next important objection is that the surroundings were unnatural. I myself long since criticised my method on these grounds,¹ and I am and always have been ready to admit that an animal may be able to reason with certain data, to imitate certain acts, and yet be unable to reason with the data with which you confront him or imitate the act you present as a model. For that reason I chose varied acts,

¹ See *Science*, Vol. VIII., No. 198, p. 520.

very simple acts, trying each with different animals and making many of them approach very closely to acts common in animal life, and making others practically identical with acts which have been recorded as proofs of high mental ability in animals (*vide* the experiments with boxes C, D and G). We have seen that so far as the mere being in boxes is concerned the animals soon got used to it, did not fear it, and presumably could and did use their mental powers while in that situation. If Professor Mills had specified some particular situation as unnatural, and argued in concrete terms that its remoteness from the ordinary conditions of animal life made it unfit to call forth what mental functions the animal had, I should here either try to show that it was fit to call them forth or confess that from the animal's conduct in it no conclusion could be drawn save the one that the animal's mentality was such as was not aroused thereby. Even this one conclusion would be valuable. Even if we had to say, 'all that these experiments prove is that these circumstances will not cause the animal to manifest memory, imitation, etc.,' we should be saying a good deal, for the advocates of the reason theory have pretty uniformly given as evidence the reactions of animals to novel mechanical continuances.

Professor Mills does not argue in concrete terms, does not criticise concrete unfitness in the situations I devised for the animals. He simply names them unnatural. Moreover, it would seem that he makes this word face two ways. When talking of my experiments, he uses the word in the sense of novel, unfamiliar to the animal. When arguing that my conclusions are wrong, he uses the word in the sense of beyond the limits of their mental functions, abhorrent to their normal intellection. Of course, the former may be true and the latter false. The fact that cats are not ordinarily treated as mine were does not imply that my cats could not and did not come to be at home in the life I imposed on them to such an extent that they could use therein all the general intellectual functions they possessed. Professor Mills himself has based statements about the presence of certain mental functions on the conduct of a kitten in gaining a certain resting-place (in a bookcase, if I remember rightly), in spite of mechanical obstacles interposed. The situation here coped with is as 'unnatural' as that in a majority of my experiments.

The general argument of the monograph is used in all sorts of scientific work and is simple enough. It says: "If dogs and cats have such and such mental functions, they will do so and so in certain situations and will not do so and so; while, on the other hand, the absence of the function in question will lead to the presence of certain

things and the absence of certain other things." To provide the 'certain situations' was the task my experiments undertook. It is mere rhetoric to damn the whole argument with a word, 'unnatural.' The thing to do is to show the error in the logic or the disturbing factor in each experiment, to repeat the experiment minus that factor, get opposite results, and so refute my claims. Dr. Kline has in one slight case gained results by the use of more 'natural' surroundings and his results agree with mine. (See *Am. J. of Psy.*, Vol. X, pp. 277-8.) I may say here that Dr. Kline has in this article treated of fear and novel surroundings as disturbing features in my experiments more discriminately, perhaps, than Professor Mills, and that this paper is intended to be an explanation which will satisfy his criticisms as well as those of the latter.

Observational records are, as I said in the review in *Science* which has already been quoted, of very great value; but the fact remains that the host of observations so far collected, including the large number of Professor Mills' own to which he refers on page 264, had not provided us with agreement about the presence of a single general function in animal consciousness that was in dispute. I tried, therefore, to devise situations in which the conduct of the animals might be really illuminating. It would seem that Professor Mills allows that if the experiments were only free from the disturbing factors we have been talking about, the conclusions reached would be probably true, for he does not criticise the logic of the deductions. Now these conclusions are so far reaching that I am reviled for even pretending to have made such important ones. But this goes to show just that the method will, if we can show that these factors are not present, or can modify the method so as to exclude them, get us somewhere psychologically. So my general plea for experiments in animal psychology is that they at least pretend to give us an explanatory psychology, and not fragments of natural history.

Finally, just as in experiments like mine you may miss the truth by some mistake you make in picking the circumstances, the situation to test the presence of a function, so in the mere observation of the habitual life of animals or the experimental regulation of their ordinary activities, you may miss the truth by mistaking instinctive for imitative acts, associative for rational acts, permanent associations for memories. For instance, Professor Mills offers in his article, as a proof of the presence of an imitative faculty, an act (p. 268) which might very possibly have been the result of the instinct to follow common to so many young animals, so far as one can judge from his account—

"a student of McGill University has communicated to me the fact that a kitten which could not be induced to jump over an object placed before it, did so only after seeing the mother do it, and after that there was no more trouble in getting it to perform the trick." We shall see that another observation, that of the dog and the tree, which Professor Mills quotes to refute me, may have suffered in the interpretation.

Of course, it is clear that the psychological story told by correct experimentation will not conflict with the story told by correct observations reported correctly at first, second or tenth hand. But I am not yet sure that any trustworthy observation about the interpretation of which there is general agreement, conflicts with the results of my observations under test conditions in such a way as to render necessary the presupposition that in them there was some vital flaw. Such refutation of them may come, but Professor Mills does not seem to have brought it.

So much in general defence of the methods I used. It may now be permitted to mention some matters of detail: Professor Mills finds in the printed report of my experiments signs of conceit and of lack of 'respect for workers of the past of any complexion.' For psychological interpretations of the sort given by Romanes and Lindsay I certainly had and have no respect, though, of course, I esteem them for their zeal. But I cannot see that the presence or absence of megalomania in me is of any interest to comparative psychology. The monograph in question was not a presentation of personal opinion, but of certain facts, the accuracy of which, and of certain impersonal inductions and deductions, the logic of which, should be attacked impersonally. The question is whether certain facts exist and what they mean, and does not concern the individual psychology of any person.

Professor Mills' humor in making believe that because I characterize Lloyd Morgan as the 'sanest' of comparative psychologists, I think of them all as insane (p. 263), seems a bit disingenuous in view of the fact that his article will probably be the sole source of information about my book to a large number of people. Of course, when I wrote 'sanest,' I meant sanest. Had I meant 'least insane' I should assuredly have so written. On page 264 our author says, 'He' (Dr. Thorndike) 'comes very near to the belief that they are automata pure and simple, though this he does not assert in so many words.' This, I may be permitted to say, is an absolute misrepresentation. In every associative process discussed in the book I find present as an important element, *impulses*, and impulse I expressly define as 'the consciousness accompanying,' etc. (p. 14). Again, I speak everywhere

of the *pleasure* resulting from the attainment of freedom, food, etc., as stamping in the connection between sense-impression and impulse. So, also, I speak everywhere of the sense-impression as the starting-point of the mental association. As a fact, *mental* processes are mentioned throughout the whole discussion. The one place where I frankly offered opinion in addition to fact was where I also attributed *representations* to animals: 'my opinion would be that animals *do* have representations, and that such are the beginning of the rich life of ideas in man' (p. 77). Again, after an attempt to 'describe graphically * * * the *mental* fact we have been studying,' I say (p. 89): "Yet there is consciousness enough at the time, keen consciousness of the sense-impressions, impulses, feelings of one's bodily acts. So with the animals. There is consciousness enough, but of this kind."

On page 264 Professor Mills talks as if I were trying to answer the question as to whether the animal mind was comparable to the human mind, and to answer it in the negative for the sake of exalting the human mind above the realm of natural evolution. The reader of the monograph will remember that one of the results of the study was the attainment of a possible mental evolution of an entirely natural sort. I never tried to answer the question, 'How far does the mentality of a dog or cat equal that of man in general, genus homo,' for such a question seems to me fruitless. It is like asking how far is z like x . The mentality of man *in general* is an unknown quantity, has a lot of possible values and so cannot be well used as a measure of anything. Any answer to it will be partially false and partially meaningless. Whether cats infer and compare, whether they imitate as present day adult human beings known to psychologists do, whether they form associations minus impulses of their own, are clear, answerable questions. Such I tried to answer. To say or to prove that the human mind of Europeans of to-day comes by continuous evolution from the animal mind does not make the latter any higher, endow it with a single new function nor alter it one whit. The protozoa are not at all different from what they were before after we call them the ancestors of the vertebrates. And one is free, it seems to me, to find out about questions of descriptive psychology, as well as of morphology, without meddling with questions of classification.

On page 265 Professor Mills rebukes me for considering hunger the strongest stimulus to animals. Of course, I did not so consider it, and I am not aware of anything in the monograph which even looks as if I did.

Again, on this same page he misrepresents me by quoting a sentence

without its context and, indeed, with comments which positively give a wrong notion of the context. The sentence is: 'the question of whether an animal does or does not form a certain association requires for an answer no higher qualification than a pair of eyes.' This sentence, as anyone may see by reading pages 5, 6 and 7 of the monograph, refers to the particular associations involved in learning to escape from boxes. And whether an animal does or does not learn to escape from a box certainly can be observed by anyone with a pair of eyes. And as the text clearly states, it was just because I did not wish to impose on any one my own opinions or even observations, because I wanted to use a method which any one else could employ and gain results which any one else could verify or refute, that I planned experiments which depended, so to speak, on impersonal eyes, eyes in general, for many of their results. I unhesitatingly affirm that so far as the facts of escape or non-escape and the time records (and the sentence concerns nothing else), Professor Mills or any one else would have kept just the same records as I myself did—that his eyes would have seen no more nor less than mine.

On page 267 I am accused of sacrificing particulars about facts for the sake of rhetoric, again on the basis of an entirely misrepresented quotation. On pages 38 and 39 of the monograph I say that henceforth I shall frequently use the word 'animal' or 'animals' when I mean to make statements only about the particular score of animals which were the subjects of my experiments, as "really I claim for my animal psychology only that it is the psychology of just these particular animals." After giving one reason for this verbal usage I add, "my second reason is that I hate to burden the reader with the disgusting rhetoric which would result if I had to insert particularizations and reservations at every step." Professor Mills quotes, omitting the first five words, and giving the impression that I generally omitted details so as to have good paragraphs or something of that sort, whereas the only 'particularizations' to which I objected were such as saying, Cats 1 (8-10 months), 2 (5-7 months), 3 (5-11 months) etc., up to cat 13; Dogs etc., etc., did not do so and so every page or two, when by means of this little note upon verbal usage the reader could on each occasion interpret the word 'animals' to mean "the particular animals which he observed, not necessarily all animals." The rhetorical excellence thus gained requires absolutely no sacrifice of fact of any sort.

If I were sure that Professor Mills would enjoy a bit of jocularity, I should reply to his explanation of the failure of my animals to imitate, by his own failure to imitate Professors James, Ladd, Hall and

Cattell, by saying that it was a good explanation, that they, like him, did not imitate because they could not. His whole discussion of my views on imitation should, in fairness, be accepted only after a careful reading of what the monograph said on that subject. There is room in this reply for only one more comment, on another matter.

To prove that dogs have memory in the sense of the ability to "refer the present situation to a situation of the past and realize that it is the same" (the meaning taken in the monograph), Professor Mills tells us of a dog which stopped at a certain tree, up which he had, months ago, chased a cat, "looked up and behaved otherwise in such a manner as left no doubt in my mind that he remembered the identical tree and detail of the whole performance." I suppose this description of the effect on Professor Mills, beginning with the words 'behaved otherwise,' means that the dog barked at or jumped at the tree, or behaved as he would if the cat were there. It must be confessed that to a hardened disbeliever the argument, "the dog remembered because he behaved so that I know he remembered," seems hardly scientific; but supposing that the description means what we have suggested, it still does not prove that the dog felt a memory of previous incident. At the table this morning I took hold of a cup, raised it to my lips and drank, acted toward the cup just as I did a month ago, but I had absolutely no memory in connection with the act. Indeed, if the dog really remembered the previous chase, he would have good reasons *not* to stop at the tree and act as if a cat were there. Let us suppose that Professor Mills and his dog were both out for cats; that they chased a cat to a tree; that the dog barked, etc., at the foot; and that Professor Mills, running up, shot his gun at the cat. Next month they come along toward the tree. Now, suppose that Professor Mills should run up and shoot his gun as he did the other time. Would we think he remembered his chase of a month before? No! we would think that he had gone daft, or had *forgotten* that the cat was there a month ago. Such an act would be the natural result of a permanent association between the sight of that tree and certain impulses, or of an ill-defined representation; but it would be one of the last things to expect as a result of a memory of the previous occasion.

This reply should close with an apology. Discussions of method and argument over results are likely to be less profitable and much less interesting than new constructive work. This reply was, however, necessary because of Professor Mills' eminence as an observer of animals, and because of the importance of getting at the truth about the

possible disturbing influence of fear and novel surroundings in certain convenient and, if legitimate, illuminating experiments.

[NOTE.—On page 268 Professor Mills has put ‘to the laws of nature’ instead of ‘to the laws of its nature,’ which means something rather different.]

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NOTES ON AFTER-IMAGES.

LOCATION OF AFTER-IMAGE.

The following *Experiment 1* was made while I studied at Princeton, January 26, 1895. With an ordinary students' stand-lamp, I closed the left eye, shaded it with the hand, and gazed steadily at the flame until an exceedingly strong image was secured. Then, closing this eye and likewise covering it with the hand, I secured a strong image with the left eye.

Then, with a large piece of cardboard the eyes were shaded from the lamp-light and the after-image of the right eye was projected upon the wall, which was of a light shade. While this image was complementing from green to red, and at just the time the red was well produced, that eye was closed and the image of the left eye was thrown upon the wall, which image was found to be green at the instant that of the right was red. In like manner, when the image of the left eye was complemented into red, and the image of the right eye was at that time found to be green. Opening and closing the eyes alternately, it was found that each eye had its own independent after-image.

Experiment 2.—Proceeding as before in securing the after-images opposite in color for the eyes, the left eye was closed and the image of the right eye was projected on the wall. When this after-image had changed to red I projected the after-image of the left eye upon that of the right, that of the left at that instant being green. The combined image appeared green. Upon closing the left eye, or upon shifting its image to the left so as to make two separate images, it was found that the image of the right continued to be red while that of the left was green. The reverse was likewise accomplished. With sufficiently strong images this shifting of images into and away from each other proved an exceedingly interesting and beautiful process.

The above experiments, if taken alone, seem to indicate quite decisively that the after-image pertains to the retina of the eye. Mr. McCurdy, who frequently studied in my room, upon being informed of this experiment, tried it and obtained the same result, and likewise felt satisfied with the evidence of retinal location.

AFTER-IMAGE AND TEMPERATURE.

The following describes what was rather an experience than an experiment, since it conducted itself, and that so impressively that I was enabled to chronicle it in detail after going to my room.

It is necessary to explain that, while studying in Chicago, I was accustomed to public speaking each Sunday evening, and finding that a double bath—that is, a hot bath succeeded by a cold one—proved beneficial toward reducing nervous excitement following on the effort of speaking, and conducive to sleep, it was habitually practised.

On an evening in March, 1898, while lying in a bath as hot as I could well endure, my eyes being closed, I noticed a very lively after-image. I presume it had been caused by looking at the gas light in the bath room, although unconsciously. Its peculiar shape and brilliancy attracted my notice so much that I became interested in its life history. Its shape was that of a heart and its color that of the gas flame recently lighted. Besides its peculiar form, another novelty was the trimming of green globules which embroidered the image. While attending this feature I became aware that the image, instead of diminishing in intensity, as becomes the normal after-image, was growing more intense and brilliant. At the time I had become so warm that perspiration stood out on my face and forehead. As I watched, the globular fringe began to shift around to one side—the left side—and to thicken there into a kind of knob. At about the same time another small after-image of exactly the same color and shape as the former image began to form in the right center of the latter. It must be noticed here that the old image persisted in remaining the same color and refused to complement itself. The second image grew rapidly; and now a strange thing took place, namely, the small image moved closer outside to the right of the older and larger, and increased to about the same size. Then both images changed position, rolled over, as it were, upon their sides, with their niches toward each other. The green fringe of globules now concentrated in each image at the niche and the two images began to coalesce. First, the marginal perimeters remained distinct between them, then merged into one separatrix, but eventually disappeared, leaving but one after-

image, with a core, as it were, in the center. At this point the image was much larger than the first image had been, and more intense than any I had ever previously observed. And, strange to say, the color persisted without complementing. That is, in general, for the color had gradually shaded into a beautiful pink, while the center was a sort of apple green. In fact, the appearance of the image at its zenith resembled a large pink candy apple with its green center toward the eye. At this time I was suffering from the heat. Turning on the cold water, the bath began gradually to cool. With the decrease of temperature the size and intensity of the after-image reduced. By the time the bath was reduced in temperature so as to feel decidedly chilly, it had entirely disappeared. The last glimpse I got as it was fast paling—there was an orange colored daub across the left center. When the water was really cold I not only could not get a return of the image, but could get but a very poor after-image by repeatedly gazing at the gas flame.

I judge from this experience that the high temperature of the bath caused a rush of blood to the periphery of the body and so to the end organs of the optic nerves, stimulating the retina, so that feeble impressions were wrought up to remarkable intensity.

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PSYCHOLOGICAL LITERATURE.

Truth and Error, or The Science of Intellection. J. W. POWELL.

Chicago, The Open Court Publishing Co.; London, Kegan Paul, Trench, Trübner & Co. 1898. Pp. 423.

It is a precious discipline for the schooled laborers in psychology and philosophy that they must submit to learn from minds that obstinately refuse to learn from them or their masters. Mr. Powell's book has the freshness, suggestion, courage and masterful quality that we associate with the thinkers who inaugurate 'periods' and who are unoppressed and unchastened by a long dismal vista of strenuous and largely ineffectual thought behind them. He has striking conceptions, and often admirable expression, but he will not learn his a, b, c's. He has the familiar refrain of condemnation for 'metaphysics' and those who are deceived by words, but his own unguarded reliance on words, joined with an arbitrary definition of them, is not encouraging. For instance, 'metaphysics' itself. "In modern times those who hold that noumena are inexplicable, that is, unknown and unknowable properties, call themselves 'metaphysicians.'" Mill, Spencer, Aristotle, Plato, Newton, Berkeley, Kant and Hegel, are freely refuted; but the author's insight into other men's thoughts and methods falls considerably below his confidence. The strictness and prudence of his own reasoning—his philosopher's conscience about assumption and assertion—might be pointedly illustrated.

'The war of philosophy,' we are told, has been 'between Idealists and Materialists,' according, of course, to Mr. Powell's definitions. "The philosophy here presented is neither Idealism nor Materialism; I would fain call it the philosophy of science." It is realistic in the modern, and, one is tempted to add, in the scholastic sense. He takes his first principles avowedly from empirical science and seems to be untroubled by the scruples of epistemology. "I shall propound the hypothesis that consciousness inheres in the ultimate particle, and attempt to show that it [*i. e.*, the hypothesis] harmonizes the principles of psychology." "An ultimate particle, and hence every body, has five essentials or concomitants, these terms being practically synonymous. *** The essentials of the particle are unity, extension, speed, per-

sistence and consciousness, which are absolute. The relations that arise from them, in order, are multeity, position, path, change and choice, which give rise to number, extension, motion, time and judgment, as properties that can be measured. It has been pointed out that particles are incorporated in bodies through affinity as choice, and by this incorporation the quantitative properties become classific properties which, in order, are class, form, force, causation and conception." Unfortunately one must content oneself with these quotations. This philosophy of science is interesting; and if somewhat remote from both modern philosophy and science, it remains true that the book abounds in suggestive statement and clever expression, and furnishes striking illustrative passages to the student of that attractive but undeveloped subject, the psychology of philosophic speculation.

D. S. MILLER.

PHILADELPHIA.

Human Immortality. WILLIAM JAMES. Boston and New York, Houghton, Mifflin & Co. 1898. Pp. 1 + 170.

This little book of Professor James' is the Ingersoll Lecture for 1898. The author seeks to answer two objections of modern culture to immortality. The first of these is the proposition of Physiological Psychology that thought is a function of the brain. This general idea has been carried into detail by the hospitals and laboratories which have located special forms of thought in special brain areas. Professor James asks us to accept this result for the sake of argument, and asks whether it compels us to surrender belief in immortality. Most persons imbued with the 'Puritanism of science,' he tells us, would answer in the affirmative. But this conclusion is not logically coercive, because the physiologist assumes that the only kind of 'functional dependence' is production, and supposes that the brain produces consciousness. But this overlooks 'permissive function' and 'transmissive function,' with which we are familiar even in the physical world. Professor James' thesis is that when we say that thought is a function of the brain, we are entitled to think of 'permissive' and 'transmissive' function. He considers the latter, which he illustrates physically by the keys of an organ, which transmit the air from the air chest through the pipes into the world, in certain special forms. Suppose that the whole universe of material things is only a 'surface veil of phenomena' hiding the reality behind, or a dome refracting the 'white light of eternity.' And suppose this dome, usually opaque to the eternal light, could at certain places grow less so, admitting to this

world so many restricted rays. Just so our brains can be conceived as thin places in the dome through which the genuine reality, the life of souls, breaks through into this world in restricted forms of finite consciousness, which would only cease in these special forms when the various brains ceased to exist. Thus our conscious life would depend on our brains, and yet an immortal life beyond the veil be possible. Professor James says in one of the notes that he takes the dualistic standpoint of natural science, because this objection arises on this plane. From this standpoint, he says, if we reject the notion that the brain produces consciousness, we have no other alternative but to believe that consciousness preëxists and is transmitted into this world of phenomena by the brains which give it its finite forms. He then shows that the idea of production is quite as metaphysical as the idea of transmission, and finally gives certain positive advantages of the transmission theory.

Several questions suggest themselves. There is space here only for two. The first has reference to the idea of 'transmission.' If it be admitted that thought is a function of the brain, and also that 'transmission' can be called a function, then Professor James might exclude all other theories than production and transmission without any refutation, on the ground that they do not make thought a function of the brain. But if the physiologist can reply that this idea of transmission of preëxisting consciousness by the brain does not make the former a function of the latter, it would seem that we have given up the proposition we were to have accepted, and the transmission theory would have to hold its own against other theories which hold an absolute beginning of the finite consciousness, such, for example, as those which say that it is created either absolutely or through the generation of parents. And if the brain passively transmits a preëxisting consciousness as a 'thin place in the dome' lets in light, it would seem as though it could not be said that thought is a function of the brain, if function is to have any intelligible sense. If this theory does not have the right any more than others to the support of the physiologist's proposition, it must take its place in the arena with the others.

A second question is suggested by the author himself: How does this theory help us to realize our finite and individual immortality? Our finiteness seems to be a part of the warp and woof of our personality; and if when the brain, the organ of this finiteness, vanishes, our spirits revert to their original source, what is to become of our personal immortality? Professor James admits that these are vital questions, but declines to enter into the discussion of what he calls

'these higher or more transcendental matters.' He merely says by the way, that if, as the philosophers say, 'all determination is negation,' it might prove that the loss of these particular determinations is not a matter for regret, and that they are not worth keeping. But what if all determination is not negation? What if those elements of our finite personalities are positive and worth keeping, and their loss a 'matter for regret?' It would then seem that this theory would not help us greatly in the question of immortality. If it is to be a valid argument for immortality it would first have to prove that 'all determination is negation.' Nor does it relieve the matter that Professor James says in a note at the end of the book that it is not necessary to identify the preëxisting consciousness, which this theory presupposes, with the Absolute of transcendental idealism. Even though the theory only requires that consciousness preëxists 'in vaster entities' than our finite spirits, we lose ourselves just as much in the bosom of these 'vaster entities' as we would in that of the Absolute.

The second objection to immortality, for which there is little space here left, has reference to the incredible number of beings which must be immortal if we hold fast to our belief in immortality. Professor James shows that this is a fallacy resulting from our failure to realize the inner significance of these alien lives, which is as great for them as that of our own for us. Moreover, we cannot say that God has no need for these lives because we ourselves have not. If God suffers us, surely we can suffer one another.

C. W. HODGE.

PRINCETON.

Essays on the Bases of the Mystic Knowledge. E. RECEJAC.
Translated by SARA CARR UPTON. New York, Charles Scribner's Sons. 1899.

This exceedingly interesting and suggestive book, which has been well rendered into English, may be characterized as a search for the Absolute through the mystic intuition. 'Reason is in possession of too much light,' the author says in his introduction, 'to be able to remain quite at ease in the region of clear ideas, but not enough to know first principles of actual knowledge. In this penumbra who can trace the exact limit of perceptions and say where the true disappears in the probable, where the probable vanishes in illusion?' The author holds the common ground of mysticism with reference to the inability of pure reason. It is impossible to grasp the highest truth by a rational act, or to reduce it to the form of definite conceptions. The Absolute can only be

grasped by a species of inspiration, and the highest truth transcends ideas and is only expressible in symbols. The author is at the same time a positivist and an agnostic, and yet denies that science is the only organ of knowledge. The 'Heart,' by which is meant a synthesis of freedom or moral spontaneity and imagination acting under the regulative categories of duty, constitutes an ultra-rational and ultra-scientific organ of truth. The relation of mysticism to science, the author argues, is purely negative. Mysticism, when it understands itself, does not encroach on the territory of scientific knowledge. It admits and leaves it to itself, and claims the power of discovering, through its own organ, truths that are inaccessible to science. The mystical object is not ontologically transcendent. The Absolute is nowhere but in consciousness. But it is to be reached only by a consciousness raised to a high degree of intensity, which, by an act of 'excess' or 'disinterestedness,' or 'self-alienation,' transcends its ordinary plane of intellection and moral egotism, and in this act of 'transcendence' becomes, for the time, identical with the Absolute and attains to supersensuous, absolute truth. This apprehension is not intellectual, however, and cannot be represented in terms of ordinary knowledge. It can only be expressed in symbols, and these must also be the creation of the excited consciousness in which the intuition takes place. The mystical symbol cannot, therefore, possess universal value, like the principles of rational knowledge. How, then, is mysticism to be guarded against enthusiasm and subjective caprice? A negative criterion arises out of the relation of mysticism to science. Mysticism must not enter the preserves of science. When it essays to occupy fields open to science it becomes false mysticism and is to be condemned. But the most important criterion is positive. The 'Heart' must be impelled by the motives of pure morality, and the result of its mystical act must submit to be judged by the laws of duty. It must be tributary to the moral good. The author is here a disciple of Kant, as he is Kant's disciple in accepting as final his condemnation of metaphysics. This symbolic knowledge, though not amenable to the tests of that which makes the claim of universality, is not without its own appropriate canons of self-criticism.

The discussion of the book is divided into three parts. In Part I., entitled "The Absolute," the problem is how the Absolute is to be apprehended; the first chapter being devoted to various defective mental attitudes toward the Absolute, while the second treats of the mystic consciousness as the only organ for the real apprehension of absolute truth. Part II., entitled "Symbols," treats of the mode of ap-

prehending and expressing the mystical intuition, while Part III., under the title of "The Heart," deals with the moral and religious aspects of mysticism. It would be impossible, in the limits which must be observed in this notice, to follow the author into any of the details of his discussion. One is impressed with the general sanity of the discussion and the fine irenic temper which pervades it, as well as with the author's intelligent appreciation of the results of modern investigation. The book embodies an attempt to bring a very recalcitrant theme within the sphere of critical treatment. The phenomena of mysticism are treated mainly from the psychologist's point of view, and it is from this standpoint chiefly, therefore, that the attempt is to be judged. The psychological interest and value of the author's work seem to me to be unquestionable, though the extent to which mystical phenomena are open to psychological treatment is a question on which difference of opinion is likely to prevail. The proposal to substitute 'Mystic Positivism' for rational theology or metaphysical idealism touches some of the great issues of the ages. Whilst free to admit my own scepticism as to the adequacy of the substitute and my persistent adherence to a larger faith in reason, I am yet of the opinion that the author has performed an important service to philosophy. To one who is foolish enough in these degenerate days to be troubled about the ultimate problems of life and destiny the book is refreshing as well as illuminating. It proves that the search for the Absolute has not yet become antiquated, and it leads one to think that philosophy may possibly have something important to learn from the mystics.

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Psychologie der Veränderungsauffassung. L. WILLIAM STERN.
Mit 15 Figuren im Text. Breslau, Preuss und Jünger. 1898.
Pp. viii + 264.

Ever since the acceptance of the dictum *Semper idem sentire ac non sentire ad idem reverunt* (Hobbes), psychologists have been searching the multiple variations which crowd in upon consciousness; and, of late, these empirical facts have had a semblance of scientific treatment in the so-called 'law of variety' (Hamilton) or 'law of relativity' (Wundt). Change as objective sequence, and change as having meaning, have given no end of trouble to clear thinking, whether in metaphysics or in science. The work under review presents itself with the avowed purpose of bringing together the facts and meaning of change from the psychological point of view.

Noting the historical and metaphysical importance of the concept of change and its congeners, the author passes to the problem which change offers to psychology, formulating it thus: "To exhibit all the forms which the apprehension [*Auffassung*] of change can assume in universal thinking (including the interrelations of them one to another) and to describe the ideational contents of these various forms of apprehension" (p. 5). Change has many different aspects: as, quantitatively, the increase or decrease of substance, the heightening or lessening the intensity of an experience, the enlargement or diminution of its extensity, the improvement or deterioration in value, and variation in rapidity of change; secondly, the type or quality of change, as transition, transformation, interchange, beginning, progression; thirdly, the local direction of change, as movement, transition, process, etc. In its highest orders change appears in the form of development, history and mathematical functions. To this descriptive problem there is attached another quite as important, which represents the last stage of differentiation that psychology reaches, viz.: the causal investigation of the nature, origin, amount and law of the apprehension of change (p. 11).

This double statement of the problem is inadequate without a definition of *Auffassung*, the second member of the title. The author finds difficulty in defining precisely what is to be included in this term. His is distinctly not the problem of the 'perception' of changes, nor of the effects of objectively changing stimuli upon the senses; but rather of the manner how a certain form of our ideation and thinking is constituted (p. 12 f.). *Auffassung* implies the complex, discriminative, psychical activity which meets all varieties of stimulation and issues in all forms of judgment (pp. 120 f., 138). The monograph is thus an extended study of sensation, perception and mental activity as complexly involving change, its fundamental thought being that the active functioning of consciousness is the only hypothesis which offers any aid for the solution of the technical difficulties involved in attempts at harmonizing our varying experiences of changes (p. 255). Changing, rather than constant, impressions alone are the conditions for awakening this functioning (p. 140). In every paragraph, almost, there is a steady mindfulness of this two-fold problem.

The monograph is divided into two parts. Part I. (pp. 19-73) treats of 'the origin,' and Part II. (pp. 78-256) of 'the fineness of the apprehension of change.' The former is purely qualitative; the latter, more quantitative in its treatment. In Chapter I. there is given a discussion of the sources of changes as found in Perception, involving the

psychological, rather than the punctual or mathematical, present in consciousness. Fixed and gradually changing stimulations, and the changes which the subject may induce in its own conscious stream and in the members of its body, serve as the varied starting-points whence change is apprehended. In every act of perception which is essentially extended in time there is an intuitive apprehension of change. Otherwise Reproduction and Comparison (Chapter II.) could not take part in bringing about those ideas which have change as their characteristic category. Feelings of recognition accompany these purely intellectual processes (p. 54). The comparison of varying changes, whether minute or extensive, is given a very high function, its greatest importance being to promote awareness of specific stadia in any series of changes which involves more than two members; as, for example, in the growth of a plant we are able to detect 'phases' now and then. This process is essentially involved in consciousness's recognition of itself and its past. (In this process, also, S. finds 'the most important psychological root' of the idea of *Ding an sich.*, p. 67.) These 'phases' are resting-points, boundary lines, which are necessary as soon as we attempt to fix the changes by thought, word or number (p. 72).

The analytical distinctions not only aid the later examination of the measurement of change, but the quantitative analysis tends to complete and reinforce them. Part II. gathers, in critical fashion, the results in this 'relatively young field of experimental psychology.' Chapter III. describes the Technique concerning the few special pieces of apparatus which have been used in measuring stimulus changes in brightness and color, in pitch, intensity and direction of tones, in pressure, thermic and other changes due to chemical and mechanical stimulation. The mechanical devices used in the studies by Preyer, Hall and Motora, von Frey, Seashore, Stratton, and by S. in his earlier studies on brightness, movement and tones, are noted in this connection. The latter part of this chapter discusses the two groups of methods of getting at the fineness of discrimination of changes, viz.: judgment and reaction. In this matter of method S. finds a field of questions that is new and unexplored (pp. 102 f.), and offers suggestions for reducing varying affirmative and negative judgments to quantitative values, which he regards as the more desirable results in experimental tests (pp. 97 f., 91).

Chapter IV., entitled Psychical Excitability for changes and its Laws, is by far the most important part of the monograph, both as to bulk (pp. 119-256) and contents. Its first section is occupied with a

search for a technical term which shall be generic to sensibility and discriminability. This term is *Erregbarkeit* (pp. 119, 124), which means the collective reaction of the soul upon external stimulation. The development of this position brings S. to a critical rejection of Preyer's view that sensation is merely a function of stimulus changes (pp. 144, 157 f.). The remainder of the chapter endeavors, by a careful examination of the conditions of psychical response to external objects (§§ 9-11), and later by a review of the modifications of these conditions in the special senses as revealed by the somewhat conflicting results of the experimental studies mentioned above (§§ 12-14), to reach a special law generalizing psychical excitability to changes. (It is noteworthy that S. does not find Weber's Law to have any great bearing upon his problem, p. 131, note.) All senses, except that of temperature, readily yield to the propositions to be mentioned later. The perception of heat and cold and their changes seem to defy the alleged uniformity of conditions for the apprehension of changes, leading to a suggested hypothesis of heat and cold being relations only. Experimental data in vision, hearing, pressure and the modifications of constant and transition-sensations due to fatigue, surprise, expectation, etc., are given a careful examination in the interests of the special formula sought to generalize the facts of the variations in the rapidity and other features with which stimulus changes affect the perceptibility of those changes. By his earlier investigations S. has entitled himself to this critical comparison of the data in this special field (PSYCH. REV., II.: 313 f., V.: 98 f.). This composite review is suggestive and appreciative, even when critical of the work of others—which is by no means uncommon.

The 'Hauptgesetz, mentioned above as the objective point of the treatment of the quantitative values, is slowly reached and presented in sections, pertinent to the specific conditions and the type of sensation and reaction. In the serial order its parts read thus: 1. "It is not the absolute value of the excitations always present in sensory nerves, and radiating from there to the motor fields, to which motor nerves respond with a reaction; but it is rather the change in this value from moment to moment" (p. 145). 2. "It is not the absolute value of the stimulus affecting motor nerves which produces a movement, but it is rather the change in this value from moment to moment" (p. 145). 3. (Physiologically) "A nerve stimulation may become a specific cause for the performance of physical and psychical activity only when the stimulation is a changing one." 4. (Or, psychologically) "A

sensation may become a specific cause for the performance of physical or psychical activity only when it is apprehended in the process of changing" (p. 158). 5. "The incitation to the physical or psychical reaction varies directly with the rapidity of the change in the sensation" (p. 211). In a series of tone discriminations, varying from $\frac{1}{4}$ to 2 vibrations, lasting 2, 4, 6 and 8 seconds, S. found that the most favorable time for detecting the amount of increase and direction of stimulation was 6 seconds' duration (pp. 189-195). On this basis he offers the law of the most favorable time for apprehending changes. 6. "If a changing stimulus is persistently observed, certain favorable stadia will be found within the observation time in which the capability of perception (the tendency to complete a judgment—or motor-reaction) is especially strong. Since, within such a favorable time changes of varying rapidity can be perceived, the slower changes which, up to that point of time have acquired only a lesser extent, are relatively more favorably placed" (p. 211). 7. (In addition to 2) "The greater the rapidity of change in the stimulus the greater is the incitation to motor activity" (p. 213). 3 and 4 express the law of change in its best forms. The actual experimental deviations from the law, noticed at length by S. (p. 224f), must be omitted.

The law is less suggestive in its formulæ than in the discussions which point the way to it. Its formulation is rather defective in being so scattered. It remains isolated, finally, and is not exactly brought into harmony with his problem as defined in terms of apprehension. And, furthermore, the attempts at emulation are rather too pronounced. On the other hand, in its review of the few experimental studies, the monograph can well serve as a hand-book. The detailed analysis of many facts and relations brings them up to the point where only special investigation can carry them forward. This is one of the most valuable features of this very circumspect treatment of the problems connected with the apprehension of change.

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Psychophysiologische Erkenntnistheorie. THEODOR ZIEHEN. Jena, Gustav Fischer. 1898. Pp. 105.

In the rush to epistemology the serious student may well question whether we are keeping ourselves aloof from a neo-scholasticism which threatens scientific method, on the one hand, and disables the efforts of thought by an apparent show of consistency on the other. Noetical theories, once unknown and unsought, are now so common that they

even serve the purpose of setting up standards for giving advice. Whether this highest court of Appeal in the kingdom of mind is not deserving of far more respect than is, by popular consent, manifest, can scarcely be a matter of question. Consistency, however, is the chief emblem of this bar, and the essay under review presents itself with titular claims to a frank hearing. It reposes upon the earlier efforts of its writer in the field of physiological psychology, and thus comes with the promise and potency of unsuspected extensions in the domain of theory of knowledge.

In all exploration, results are in primary demand; and the author has spared the reviewer much by bringing in one section (§ 22, pp. 100-103) a 'dogmatic résumé' of his inquiries. Though opposed to the very spirit of epistemology (which the author recognizes), he ventures to give a succinct exhibition of the conclusions to which he has been led in his analyses. Freely rendered, this '*Lexikon*,' *so zu sagen*, of his theory of knowledge runs about thus:

"Sensations [*Empfindungen*] and ideas [*Vorstellungen*] are given to us." Both are summed up in the terms psychical processes, or the psychical. Non-psychical is a meaningless term. Things, my *ego*, *alter egoes* are ideas only.

On the basis of epistemological analysis each sensation is made up of two components, the residual factor, or the reduced sensation, and the *v*-component. [The '*v*' factor is the primary experience derived from the activity of the sense-organs, as tactile, visual, etc., p. 22]. The former factors have reciprocal relations, which can be expressed by universal laws. The totality of these laws is designated as the 'causal formula.' [This is the summary of the coördination of tactile and optical series mutually dependent—*e. g.*, in seeing my hand movements and pen movements spatially and temporally connected. This is the sequence of value to the natural sciences, p. 25.]

A certain group of sensations is designated as the group of *v*-sensations. The residual factors of these sensations work first reciprocally with the residual factors of the other sensations which can be expressed by the causal formula; but, secondly, they react independently upon these residual factors when they have undergone a change through the residual factors of another sensation.

These reactions are not arranged according to space and time. They cannot be expressed by the laws of the causal formula, but rather, in their entirety, by other laws (uniform fusions). The totality of these lawful fusions is designated by the term parallel-formula [*i. e.*, changes in sensation-complexes that are simultaneous and not successive, pp. 25-6].

The residual factor of a sensation, which causally effects in a certain degree and certain quality the constituent part of the ν -sensation is altered by the parallel-reaction of the latter. The process of transition is designated by the term ν -change, or individualization, the change itself as the ν -component.

Epistemology is trying, with the help of natural science, including psychology, to eliminate these ν -components, and to present the residual factor itself [*i. e.*, the 'object']. The idea of this residual factor is the resultant idea, or the reduced sensation. (Hence) the method of theory of knowledge is called 'reduction' [*i. e.*, thought must not turn to 'things' as external, but continue a consistent analysis of sensations, until it reaches that sensational *quale* which cannot be eliminated, p. 31 *f.* This residue is the nervous system, at times fibers, and again cortical centers, pp. 35, 59, 65]. The residual factors are only to be ideated.

The ν -sensations are arranged in complexes, which are commonly called sensory nervous system.

All sensations are made up of ν -components of a single complex of ν -sensations; so far forth they are individually psychical; the residual factors can also be ideated only as psychical, but as universally-psychical.

Since the idea of the individual *ego* is itself an idea resulting from the process of reduction, the idea of a universal-psychical is neither contradictory nor meaningless.

Sensations, in so far as they all possess ν -components, are also called object-sensations or stimulus-sensations.

With the omission of the ν -components the object-sensation disappears. Its residual factor must be ideated as abiding.

Every reduction factor transforms itself into so many object-sensations—*i. e.*, is individualized just so often as it affects the ν -sensation-complexes and experiences their reactions.

All sensations are positively only in space, at the place of the residual factor. Spatial and temporal series belong primarily to the reduced sensations (*i. e.*, the residual factor). It is influenced only secondarily by individualization.

In like manner, the quality and intensity of object-sensations are determined by the reduced sensations, and only secondarily by the ν -components. Theory of knowledge, in so far as it demands universal ideas of reduction, is trying, with the help of natural science and psychology, to subsume the qualities and intensities under a single reduction-idea. As such a universal the idea of energy is to-day com-

ing under consideration. The idea of mass, inasmuch as it is to indicate more than a numerical factor, is contradictory or meaningless [*i. e.*, 'mass' does not carry one over into realism].

An affective tone, as a feature independent of the other characteristics, does not belong to the reduced sensation.

The difference between ideas and sensations consists in sensuous vivacity.

* * * * *

Object-sensations always require, for their individualization, conjunction with ideas.

Ideas [like sensations] are simple or complex, individual or universal. [Ideas are only memory-pictures of sensations, p. 37.]

Ideas of Relation form a special group. They are just as dependent upon the ν -sensations as are the other ideas. Like all other ideas, they are developed only from sensations.

Among these ideas of relation, those which have special epistemological importance are the six categorical ideas [not in the Kantian or Hegelian sense of the term 'category'] of likeness, similarity and difference, persistency, change and interchange. [*I. e.*, upon the sole condition of like sensations, simultaneous and in sequence, etc., the child builds up these rational ideas, by extracting those elements present in the varying sensation-complexes, pp. 7-15.] The relational idea of causality is based upon the relational idea of change. The reduction ideas of epistemology are the most universal ideas of sensations and sensation-relations.

In the formation of epistemological reduction-ideas the regulative principle is so to plan the reductions that a general similarity appears in the place of single similarities of the object-sensations and their changes. Our reduction-ideas are subject to a progressive development and selection, since object-sensations are never given us in their totality.

Ideas of ideas, hence ideas of reduction-ideas, do not exist.

Those reductions which unite with the epistemological basis are, in so far as the latter does not change, not further in need of reduction.

The causal changes within the ν -complexes are often continued through the reduction-factors of sensations which are closely connected, spatially, with these ν -complexes. They are designated as μ -sensations, and correspond to the motor system of our bodies. These μ -complexes affect, in their turn, according to the causal formula, the reduction-factors of ordinary object-sensations. These influences are called 'actions'; and they complete the circle of causal changes."

"Can there any good thing come out of Nazareth?" is a question pointing to an active field in philosophical inquiries. Ziehen replies, as the newly adopted apostle of old, 'come and see,' while he attempts to draw noetical values out of Nazarene sensations, sensation-complexes and sequent images. If there is any virtue in the doctrine of sensation, as it has of late decades crept into science and philosophy, then this essay is the most virtuous of all recent productions which aspire to truth and consistency. The specific problem of epistemology is presented as the merely *analytical* 'reduction' of the content of experience to certain forms, validated for the means and ends of empirical sciences, especially of empirical psychology. 'Reduction' may mean transformation, fusion, synthesis. In this writing it is presented as the chief and, in truth, only means of noetical achievement (§ 8 pp. 31-35). It purports to be such an analysis of sensations and the elimination of those accidental factors which leads the naïve thinker to affirm 'things,' and the scientist to end with extra-psychical forces. This gives rise to antinomies, which it is the special problem of noëtics to remove, and, in this instance, succeeds to the satisfaction of one person in rendering all factors and processes psychical. To this extent the author is almost mortgaged to a prejudice, while the reviewer frankly confesses to the privileges in the very opposite direction—namely, that an objective, impersonal analysis of psychical contents of the lowest, or, perhaps better, initial, order is not paramount to the demands which can and must be made upon any serious attempt to explicate the nature of what we men call 'knowledge.'

That the analysis here spread forth in an exceedingly abstract, schematic, algebraic fashion is astutely regarded as adequate, may be seen from the following passages, which well illustrate the flavor of the essay, if not its detailed method of analysis:

"A special 'function of judging' does not exist." For centuries, psychology, logic, and theory of knowledge have fruitlessly attempted to find in the features of judgment what is here for the first time clearly provided for in those preliminary fusional abstractions which engage simple ideas. "There is still less occasion for accepting other 'faculties,' beyond judgment * * * such as reason, the source of syllogisms or intuitions, etc. Exactly at the place where philosophy has so often ventured the leap from epistemology into metaphysics, is the Calvary of the many 'higher functions of soul'; here lie the *λόγος* and *νοῦς* and *φρόνησις* and *μανία* and *πίστις* of Greek philosophy * * * and the reasons and pure ego's and apperceptions of

modern philosophy. It is generally supposed that a connection with the extra-psychical must be sought for in the highest psychical faculties. This extra-psychical is a senseless term, and the highest psychical is already included in the formation of ideas [as detailed by the elaborated schemata]. Right here is the principal divergence of this theory of knowledge from the pathway of the earlier theories." The scope and duty of epistemology are thus greatly modified and simplified. We are no longer concerned "with finding the criteria of true judgment, of certainty, or whatever one may term it; but only with feeling our sensations and ideating our ideas with others, and with forming new combined general and relation-ideas, and among these specially reduction-ideas which correspond to the sensations. The only criterion is the agreement with sensations, the fulfillment of the expectations which become joined to the reduction-ideas" (pp. 85-87).

The somewhat more readily assimilated results of this 'new' theory of knowledge, based upon and strictly held down to the nervous system and its initial processes in consciousness, are neatly presented in the closing section (pp. 103-105) which anticipates (rightly) and attempts to set at rest 'the almost instinctive objections' which are put to his analyses. 1. If it is supposed that the 'residual factor' of this theory is identical with 'matter,' in the ordinary sense of the term, it is replied that 'matter' is a meaningless, metaphysical dogma, and, strictly speaking, the 'residual factor' is an idea which we employ in order to reach universal laws relating our sensations and ideas. The only aspect common to 'matter' and the 'residual factor' is 'change according to universal laws.' 2. If it is presumed that one result of this reduction of things and ego's to a world of purely psychical process is to undermine all laws from that of gravitation to that of electro-magnetic light, the objector is told that their validity remains unchanged. Their labels only are changed, and our manner of speech is altered so as to avoid all contradictory and meaningless terms. 3. If the critic fancies that inroads are thus made upon the principle of psycho-physical parallelism, he is reminded that 'the psychical series alone is given,' and is the only view which avoids the specious wit involved in the affirmation of two unlike but equally persistent series (Cf. his *Introd. to Phys. Psych.*, p. 301f. (Eng. tr.)). 4. Almost overcome, the critic finally gasps for the reinstatement of 'Metaphysics, the *a priori*, the forms of Intuition, the Categories!' He is left to get his assurance in the reply that "no room remains for them. We must limit ourselves to this procedure, to gather, compare and then reduce sensations scientifically in order to attain the

universal ideas of their relations. This labor is divided between the descriptive and mathematical natural sciences, psychology and theory of knowledge. Metaphysics, just as religion, has been only the historical precursor of these sciences. It would be better to relegate metaphysics and its younger sister, metapsychic, among the fine arts." 5. "And these circumlocutory designations"—must we introduce them into daily speech, instead of the simple terms of our mother-tongue? —*e. g.*, using 'the residual factor of tree-sensation' instead of 'tree'? No. The exposition of epistemology is sesquipedalian for the very prosaic purpose 'of keeping removed those so often falsely added ideas' of realities! Thus the objector is bade to rest agnostically and nihilistically on the quiet bed of positivism, which simplifies ends and means without measure.

The spirit of this essay is extremely serious in its efforts to reduce objects by a bare-handed treatment of sensations, which are now identified with events in nerve fibers and ganglionic cells, and now with events in consciousness. There is an avowed attempt to make epistemology grow out of the soil of empirical psychology, as understood by the author in his *Leitfaden* of some nine years ago, *bien entendu*. What are the exact relations between the two branches of thought is not readily ascertained. Throughout the entire exposition in detail there is no advance beyond what *ought* to be treated under psychology as it actually is understood by most contributors to this field. Definite statements (pp. 4, 11, 58, 61, 65, 74, 85) are not steadfastly explicit upon this point. Psychology enumerates, analyzes and exhibits the development of our complex ideas, while theory of knowledge selects this or that idea, and presents the development of that which is significant for its purpose, only in so far as empirical psychology has not solved the problem. The basis of that selection, as personal, or logical, or objective, is not made plain. Again (p. 75), in tracing the formation of ideas of relation (which are not to be identified with the concepts employed in logical theory) the descent is made to the idea of 'sameness' as the given datum. The 'comparison' necessary, upon the repetition of similar sensations, is a gratuitous assumption; for it is frankly admitted that mere description is all that can be undertaken in this region of the inexplicable. On the whole, then, epistemology for the author means going back, here and there, of his specific psychological conclusions and endeavoring to make them intelligible by a further process of 'reduction' instituted in securing them. In this sense the essay is radically defective in not establishing, in a more clear-cut fashion, those differences in the two disciplines of which he gives promise in the beginning.

The tone of the work is not the most generous. It reveals an utter complacency in its Berkeleyan idealism upon a sensory foundation (pp. 5, 59). The ingenuous, at times, and severely schematic treatment sweeps aside, in almost ruthless fashion, 'the insights' of other thinkers and 'the demands' of the problem of thought and reality as perceived by them. In the light of its initial claims perhaps this procedure is commendable. One feature of this analysis of knowledge is to simplify to the grade of algebraic imagination the erstwhile serious tasks of philosophical reflection. Thus it becomes an exceedingly pertinent question to ask how this literalistic chart would fare when confronting actual knowledge. Would one recognize and identify his cognitions on the basis of the analytic and explanatory clues offered in this essay? Indeed, there is lacking that admiration for the fact of knowledge, even on a neuro-sensory basis, which every analyst *ought* to feel. Another interesting feature in this essay is the attempt to square its results with those of Kant's theory of knowledge, implying the general validity of the Kantian position as an abstract expression of truth, which finds proper treatment only in the concrete (?) elaboration of this theory of knowledge. (Cf. pp. 50+, 53-57, 72-86.) In this fashion there are repeated claims as to the exceeding advantage of this exposition over that of others.

The central question, perhaps, incited by this attempt at a theory of knowledge, which does not advance much beyond a detailed analysis of sensation-complexes (tactile, optical, and motor coming in for almost exclusive attention) is this: Can an historical analysis of certain of our residual experiences satisfy even a scientific study of what we find men calling 'knowledge'? The *Zielstrebigkeit* characteristic of every cognitive construction of an 'object' is a psychological exhibit which the epistemologist is compelled to recognize. (Cf. Baldwin, *Ment. Develop., Soc. and Eth. Int.*, pp. 249 f., 377.) It often is not the point *from* which, but the point *to* which, cognition tends that is the essential feature. This is more than primary motor-reactions (*Ziehen.*, p. 875f), and must be adequately recognized. Other constructive tendencies might be pointed out which all cognition exhibits, but of these the essay takes no notice. The benefits of the task of this essay, then, is twofold: one, in indicating the fact that on *some* assumptions a theory of knowledge leads itself into a blind-alley, cutting off further philosophical progress; the other, a benefit in awakening our thankfulness for being shown the limitations imposed by its methods and content.

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John Stuart Mill. Correspondance inédite avec Gustave D'Eichthal. Avant-propos et traduction par EUGÉNE D'EICHTAL. Paris, Félix Alcan. 1898.

The correspondence between Mill and D'Eichthal is in these pages given to the public for the first time in a complete form. Many of the letters had been published previously in the *Cosmopolis*; additional ones, however, have found a place in the volume, together with two letters of Eytoun Tooke to D'Eichthal. The friendship of Mill and D'Eichthal, which continued through a period of some forty years or more, presents many features of a most interesting nature, as disclosed in this correspondence. To the student of psychology an opportunity is afforded of noting the effect of an emotional temperament, as that of D'Eichthal, upon a coldly intellectual nature, as that of Mill, and, also, of observing the marked contrast between the French and English traits of mind. In these letters Mill exposes to a searching criticism the doctrines of Saint Simon as expounded by D'Eichthal and his friends; there is, however, a growing appreciation of the motives and purposes of the Saint Simonian school, evidently induced by the disinterested labors and self-sacrificing zeal of its members. While criticising their methods, Mill had only words of praise for the high humanitarian ideals of this school.

The strain of deep sentiment, which was a characteristic feature of Mill's nature, and yet hidden from the view of the world, is revealed in the letters which he wrote at the time of Tooke's tragic death, as also upon the occasion of the death of D'Eichthal's father. The correspondence, indeed, serves as a valuable appendix to the *Autobiography*, inasmuch as it throws additional light upon the inner life of the great logician, disclosing in that many-sided nature the elements which prove his love of humanity as well as his love of truth.

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GENERAL.

Social Automatism and the Imitation Theory. B. BOSANQUET. Mind, No. 30, N. S., April, 1899, p. 167.

The writer aims to point out, in this way-side preface to a forthcoming book, a fundamental error in the imitation theory of sociological psychology as an attempt to reduce to principle the behavior of individuals in a group. Secondary automatism suggests an analogy which throws light on political philosophy. Social life is necessarily

and increasingly constituted by adjustments which have become automatic, and are thus put beyond the range of discussion. In the resulting economy of attention the social mind is set free for new ideas. The routine of civic life, the use by the state of coercion upon the individual, and the function of punishment to awaken attention are explained in terms of this automatism. The biological principle of 'short cuts' is given application in tracing the transformations of stimuli and reacting apparatus in the world of volition. In society phenomena of identity and phenomena of difference are at once of prime importance, as is evidenced by the principles of imitation and invention, active under the forms of habit and accommodation. B. finds that repetition and similarity are only superficial characteristics of the true operative nature of social unity. No differentiation can be got out of the tendency to reproduce a copy *per se*. To introduce 'invention,' as explanatory leaves an awkward dualism. Baldwin's analysis of mental development is regarded rather as failing in its resolute repudiation of this dualism. The root of this, and other similar failures, is traced to a fallacy introduced by the influences of the atomic doctrine of association, or the repetition of similar units. Baldwin, in attempting to remould the theory, strains the idea of imitation by extending it to cover volition—the passing of an idea into fact, instead of limiting the process to mere reproduction of a copy. Nothing of serious importance happens by genuine imitation. All the business of society goes on by differentiated reactions. Every man in society is what he is through a law or scheme which assigns him an individual position, differing from all others, and identified with them precisely through these differences, by which alone he can coöperate with them. The error in question springs from working with similarity instead of identity (of factors and processes). Directly we introduce identity, difference falls into its place as an inherent aspect of the principle. Every action, without any exception, is, in principle, a difference within an identity. Relative Suggestion is a more adequate view of identity than Associationism, and B. finds in Baldwin's later writings a tendency toward the former.

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The Nature of Judgment. G. E. MOORE. Mind, No. 30, N. S., April, 1899, p. 176.

This article suggests a theory of perception and knowledge which has avowedly much in common with Kant, differing chiefly in substituting for sensations, as the date of knowledge, concepts; and in re-

fusing to regard the relations in which they stand as, in some obscure sense, the work of the mind. The view which inclines to take the categorical judgment as the typical form, and attempts in consequence to reduce the hypothetical judgment to it, is attacked. A judgment is universally a necessary combination of concepts, equally necessary whether it be true or false. It must be either true or false; but its truth or falsehood cannot depend on its relation to anything else whatever—reality, for instance, or the world in space or time. Both of these must be supposed to exist, in some sense, if the truth of our judgment is to depend upon them; and then it turns out that the truth of our judgment depends not on them, but on the judgment that they, being such and such, exist. The truth or falsehood of this judgment must be immediate properties of its own, not dependent upon any relation it may have to something else. The existential judgment, which is presupposed in Kant's reference to experience, or in Bradley's reference to reality, remains merely a necessary combination of concepts, for the necessity of which we can seek no ground, and which cannot be explained as an attribute to 'the given.' A concept is not in any intelligible sense an 'adjective,' as if there were something substantive, more ultimate than it. It is not a mental fact, nor any part of a mental fact. Concepts are possible objects of thoughts; they may come into relation with a thinker; and in order that they *may* do anything, they must already *be* something. It is indifferent to their nature whether anybody thinks them or not. They are incapable of change; and the relation into which they enter with the knowing subject implies no action or reaction. It is a unique relation which can begin or cease with a change in the subject; but the concept is neither cause nor effect of such a change. It is of such entities as these that a proposition is composed. The difference between a concept and a proposition, in virtue of which the latter alone can be called true or false, would seem to lie merely in the simplicity of the former. What kind of relation makes a proposition true, what false, cannot be further defined, but must be immediately recognized. Existential propositions do not escape this description. We must regard the whole world as formed of concepts, these being our only objects of knowledge. Perception is to be regarded philosophically as the cognition of an existential proposition, and thus it furnishes a basis for inference. From this description of a judgment there must, then, disappear all reference either to our mind or to the world. Neither of these can furnish 'ground' for anything, save in so far as they are complex judgments.

EDWARD FRANKLIN BUCHNER.

Time as related to Causality and to Space. MARY WHITON CALKINS. *Mind*, No. 30, N. S., April, 1899, p. 216.

The phenomenal unity of different kinds of multiplicity is traced to the relations of time as controlling the categorical relations of causality and space. Heretofore, time and space have been treated in the same breath, much to the misfortune of each. Analogy is not taken as a guide in the treatment of the categorical complexities involved. The thesis of the paper is the assertion that time and causality are subordinate forms of the principle of the necessary connection of phenomena, and that the third and coördinate form of the category is reciprocal determination, not, as is often stated, space. Succession, and not duration, must be admitted as constituting the nature of the temporal manifold. The synthesis of manifoldness follows fundamental distinctions, involving two sorts of necessity: first, the dependence of synthesis in general upon ultimate unity; and second, of the moment upon the preceding moment. In this way it may be seen that time really belongs among the categories, as the irreversible connection of the irrevocable, relatively abstract manifold. The psychology of time-consciousness verifies the metaphysical doctrine. The awareness of more-than-one, possessing an inner connection, presents unanalyzable elements given immediately in consciousness. The causality connection is more easily applied to outer than to inner life, and thus remains subject to the temporal sequence. The spatial sequence is no fundamental category, or uniting principle, but itself one variety of the manifold to-be-categorized. Space, as a sense-quality or a notion, is clearly a construct of experience.

EDWARD FRANKLIN BUCHNER.

SCHOOL OF PEDAGOGY,
NEW YORK UNIVERSITY.

L'Éducation des Sentiments. P. FELIX THOMAS. Paris, Alcan. 1899, p. 287.

The author, who has appeared before the public in other writings on philosophical and pedagogical subjects, presents us here with an analysis of the sentiments and emotions with pedagogical hints and suggestions as to the best methods of utilizing them. He combats the tendency in education toward excessive emphasis of the intellectual, and pleads the cause of the algedonic and volitional elements in our nature. Intellectualism tends to destroy the will and the pleasure-pain values of life. It is conduct and emotional value which makes life worth living, not creeds religious, philosophical or scientific. Pain

and pleasure depend upon the laws of vital rhythm. The appetites, desires, anger, fear, play, instinct of proprietorship, love of domination, curiosity, sympathy, pity, social inclinations, self-love, etc., are treated in turn.

The style is literary rather than scientific. Some very good suggestions are made, and the author generally strikes the right keynote in a happy manner. There is little justification for the neglect of recent American contributions on the same subjects. There is a good table of contents, but no index.

ARTHUR ALLIN.

UNIVERSITY OF COLORADO.

Il metodo deduttivo come strumento di ricerca. GIOVANNI VAILATI.
Turin, Roux, Frasati & Co. 1898. Pp. 44.

Alcune osservazioni sulle questioni di parole nella storia della scienza e della cultura. GIOVANNI VAILATI. Turin, Frat. Bocca. 1899. Pp. 39.

These two papers are introductory lectures in a course on the History of Mechanics, delivered by the author in the University of Turin. In the first Dr. Vailati discusses the value of the deductive method; he examines the history of discovery in mechanics, from Galileo down, and insists that many of its more important laws 'would still be unknown to man, at least in their generalized form, if he had not at his disposal another method besides that of observation and direct measurement.' Admitting the supremacy of induction, as a means of scientific discovery, the author, nevertheless, points out the important rôle that deductive reasoning has played from an historical standpoint. He believes that Bacon's diatribes anent the sterility of Aristotle's dialectic and the syllogism, were called forth by the excessive use of deductive methods in scholastic times, and would have been modified had induction received proper recognition in those days. The latter part of the paper is a discussion of the practical application of the various forms of induction and deduction to the discovery of scientific laws.

The second paper takes up the question of terminology in its relation to objective truth and the history of scientific thought. Dr. Vailati refers to the undue stress sometimes laid on the etymological significance of a word. He denies the *objective* importance of the distinction between definable and indefinable terms. The impossibility of defining a term may be due to the simplicity of the notion, as well as to its obscurity; in either case it is a subjective factor that distin-

guishes it from a definable term. The author works out for the benefit of his pupils a number of well-known principles underlying scientific discovery and the definition of concepts.

HOWARD C. WARREN.

PRINCETON UNIVERSITY.

Pensare senza coscienza. G. SERGI. (Reprinted.) La Rivista Moderna, Vol. II, Fasc. I. 1899. Pp. 18.

This is an amplification and in some respects an advance on the author's doctrine of the unconscious, as developed in his *Psychologie physiologique*. Professor Sergi starts out with the view that conscious thought is merely the last term in a series of unconscious brain states. In support of this theory he cites, from his own experience and others', numerous examples in which a problem has been solved or a train of reasoning worked out to a conclusion while the mind was occupied with something entirely different. In some instances the process extended over an hour, in others over a day, week, month or more. In his own case he finds many instances of this unconscious brain work proceeding during sleep; at one time it was so pronounced that he grew accustomed to read up the theme of any paper he was to write, and then immediately dismiss the whole question from his mind, without working out the plan of the paper; in the morning he would begin the writing at once with no hesitation or difficulty, the subject having apparently been analyzed and arranged for treatment during the night.

Professor Sergi reviews the theories of Kant, Leibnitz, Hamilton, J. S. Mill and Carpenter, on obscure ideas, subconsciousness and unconscious cerebration. He gives preference to Hamilton's view, that 'latent agencies—modifications of which we are unconscious—must be admitted as a groundwork of Phenomenology of Mind.' The author, however, goes further, holding that this "unconscious cerebral and physiological work constitutes the whole phenomenon, not merely one side of it, and that the consciousness of the phenomenon is merely its superficial revelation, which adds nothing to the essence and completeness of the phenomenon in question." He claims to solve the problem of psychological dualism, by making the physiological process the sole 'essence' and the state of consciousness a mere 'manifestation'. In spite of the brevity of the paper and the lack of novelty in its standpoint, it calls for attention on account of the new observational data which the author has brought forward.

HOWARD C. WARREN.

PRINCETON UNIVERSITY.

Individual Memories. F.W. COLGROVE. American Journal of Psychology, X., 1899, pp. 1-29.

This dissertation is based upon the returns of the Clark University questionnaires, and the results are presented in the usual form. 1,658 replies were tabulated upon a roll of paper one foot eight inches wide and fifty-two feet long, after almost incessant labor for five months. A second tabulation followed, grouping replies under more than sixty different headings. We admire the patience of the writer's wife, who did all this work, but we fail to discover any great value in the results. Absolutely no attention is given to the degree of certainty of the various conclusions; we are told that the males have the greatest number of memories for protracted or repeated occurrences, people, and clothing, and that they excel in topographical and logical memories, while females have better memories for novel occurrences and single impressions, for Christmas gifts and dolls, without a single figure to back up the important statement.

In the same way we are told that Indians find shorthand helpful to memory, and so on throughout the various subdivisions under the thirteen questions.

Nothing is said as to the class of people from whom the replies were collected, the ages of those questioned, or about the seriousness with which the answers were written.

C. B. BLISS.

Schmeckversuche an einzelnen Papillen. F. KIESOW. Philos. Stud., XIV, 4, 591-615.

This article gives an account of experiments, a continuation of work by Oehrwall, to discover whether or not the single taste papille reacted only to certain taste substances. Thirty-nine points on the tongues of two subjects were tested. Of these, four gave no reaction to salt, sugar, acid or quinine solutions; seven others gave characteristic tastes of each of the stimuli; one reacted only to sugar and another only to quinine. Of the remainder 19 + 5 (?) = doubtful reacted to sugar, 11 + 13 (?) to salt, 11 + 11 (?) to acid, and 6 + 8 (?) to quinine. Mechanical and electrical stimuli were used by the author, but the results are left for a later article.

SHEPHERD IVORY FRANZ.

COLUMBIA UNIVERSITY.

GENERAL.

Ueber die Auffassung einfacher Raumformen. RICHARD SEY-

FERT. Philos. Stud., XIV., 4, 550-566.

This research was an attempt to discover some of the factors influencing the accuracy of reproduction (judgment) of simple geometrical figures. Various triangles were shown for a time under different conditions, and, after a few seconds, the subject attempted to reproduce the same.

The six following conditions were used: (1) Eyes fixed upon a point within the triangle; (2) eyes followed a point which described the sides of the (imaginary) figure; (3) the eyes were shut and the finger was moved over the sides of the triangle; (4) the triangle was looked at and the eyes were moved over its contour as in (2); (5) the eyes and finger were made to describe the sides of an imagined triangle; (6) the eyes followed and the finger described the form of a seen triangle.

Owing to the varied ability and training in drawing, this simple method (by drawing) of reproduction was not used. After the figure was shown or felt, the subject was given a card on which was drawn a base; on this he was instructed to mark with a pin the apex, and from this point sides were drawn to the extremities of the base and the angular errors were noted.

From the results of nine subjects the author concludes: (1) The decisive factor for accuracy of reproduction of simple forms is not the retinal image, but the sensation of eye-movements. The most exact reproduction of such forms occurs when the eye sees the figure as a whole and follows its outline. (2) Pure eye-movements without the image of the form, are next for exactness of the reproduction. With practiced subjects this kind of reproduction equals the first in exactness. (3) The perception with fixed eyes is very puzzling, and successful only for practiced individuals. With unpracticed subjects it is almost impossible to prevent the eyes following the outline of the object. (4) Simultaneous movements of the hand and eye ('und des Auges' not 'und des Armes', see pp. 558 and 560) as a rule lessen the accuracy of reproduction. Great practice of the muscles can increase the accuracy. (5) The least exact method is reproduction from pure hand and arm movements.

Horizontal and vertical errors in placing the apex of the figure would give similar results. These errors the author has not attempted to separate.

SHEPHERD IVORY FRANZ.

COLUMBIA UNIVERSITY.

Bemerkungen über Kinderzeichnungen. KARL PAPPENHEIM.
Zeitschrift für pädagogisch Psychologie, I., pp. 57-73.

This is a review of the different studies made of children's drawings chiefly in America. It covers methods of study of children who show special aptitude, the origin of types, the different stages of development, the relation of drawings to memory, observation and language. The use of drawing in the teaching of botany, geography and zoölogy is supported.

Heredity and Environment. A Study in Adolescence. EDGAR JAMES SWIFT. American Physical Education Review, 1898, pp. 8.
Reflex Neuroses in Children. EDGAR JAMES SWIFT. American Physical Education Review, 1899, pp. 8.

The first address describes a series of questions asked of Reform School boys, about the causes which had brought them into trouble. The results, though not decisive, in many cases offer good suggestions for further work.

The second address calls attention to the fact that defects of the eye, ear, or nose, are often causes of dullness in school children.

C. B. BLISS.

VISION.

Wahrnehmungen mit einem einzelnen Zapfen der Netzhaut. G. F. SCHOUTE. Ztsch. f. Psych. u. Physiol. der Sinnesorgane, XIX., 251-263.

The author of this interesting paper shows, in opposition to Asher (Ztsch. f. Biol., XXXV., 400), that it is perfectly possible to throw upon the retina an image whose diameter is less than that of a single cone. This is important, because it removes any doubt that may have been felt as to the validity of the demonstration, by Hering and by König, that a minute point of white light is *not* seen to be now red, now green and now blue, as it falls now upon one and now upon another of the retinal cones. This demonstration gives an experimental death-blow to any three-fiber theory, and in consequence no such theory has of late years been upheld by any one. Holmgren's experiments of an opposite bearing have failed to win acceptance.

Schoute finds that he can distinguish no less than eight different sizes in small bright objects when their images are, even the largest of them, so small as to fall upon the top of a single cone. Such facts as this have hitherto been explained by supposing that, though no differ-

ence can be perceived in the images themselves, there is nevertheless sufficient light in their diffusion circles to enable the judgment to distinguish between their different sizes.

Schoute shows by ingenious experiments that this is not the source of the distinction, but that it rests simply upon the difference in the amount of luminosity. Within the range of these small dimensions, a given object cannot be distinguished from another which is both smaller and brighter. If a larger amount of light falls upon a given cone, we have no means of knowing whether it comes from a larger or from a brighter object, but because we are far more interested, in general, in the size of objects than in slight differences in their brightness, and hence make a far greater number of judgments of this nature than of the other, we here interpret an ambiguous difference in sensation as a difference of that character which stands for more to us. (In these small images we have also no means of distinguishing shape, and hence all such objects appear to us to be of the simplest shape or round.) The proposition is thus established that for images which fall upon a single cone, the judgment as to size is determined by the product of surface and intensity of light, as has been shown before, in fact, by Ricio (*Ann. d'Ottalmol.*, 1877). Asher's error was an error of method; he looked at a minute object with a microscopic arrangement of lenses, and found that for different degrees of diminution of its image, it always appeared to be of the same size; but this is merely what was to be expected, in the light of present results, for the quantity of light thrown upon the single cone was in each case the same. Schoute makes the curious observation that when the image of an object covers more than two cones, he has the distinct feeling of basing his judgment as to size upon the pure sensation of extension, that with equal certainty he feels that he is guided by difference in brightness alone when the image falls upon one cone only, and that when it is of just the size of two cones he finds his judgment wavering, so that he cannot say with certainty whether he is judging of size from brightness or from the extent of the image.

C. L. FRANKLIN.

BALTIMORE.

Subjective Colors and the After-images: their Significance for the Theory of Attention. MARGARET F. WASHBURN. *Mind*, N. S., 29, January, 1899.

Professor Washburn reports with admirable clearness the results of a series of experiments upon the possibility of influencing the suc-

sion of colors in after-images by the vivid *image* of a color. The most important outcome of the paper is summarized in the statement which follows:

The conditions under which the after-images were obtained were those suggested by Helmholtz in the *Physiologische Optik*. The subjects, of whom there were four, fixated, for twenty seconds, one point of an upper window frame; their eyes were then closed and covered, and they noted the sequence of colors of the after-images. The subjects were practiced until this order became invariable; they were then directed 'by an effort of will' to 'turn the image red all through its course.' Similar suggestions were made in regard to blue and to green. These suggestions were almost invariably effective, either by intensifying 'the traces of color already present in the field,' or by lengthening or anticipating the time of a suggested color, which normally occurred in the after-image series. Thus, a subject whose ordinary sequence of colors in the series of after-images was 'blue-positive, green-positive, red-negative, dark-blue-negative,' when asked to visualize red had the following series of color changes: 'first, a red image with dark lines, interrupted once by a momentary green image; the dark lines then became bright and the red negative image remained until the end of the series, traces of the blue appearing from time to time.'

The most evident inference from these results is the identity, for psychology, of percept and image—of sensations peripherally and centrally aroused. These experiments, therefore, though so distinct in subject-matter, strengthen the conclusions from Dr. Washburn's earlier study of the effect of visual images upon cutaneous localization.

The results are also considered in their relation to the doctrine of attention. 'The effort to call up subjectively a certain color meant,' at least for the three subjects of moderate visualizing power, 'simply an unusually intense effort to attend to that color.' But the result of this attention was an actual increase of the intensity and duration of a peripherally excited sense-experience, and it follows that the "function of attention is positive as well as negative, intensifying as well as inhibiting."

A criticism is added of Wundt's theory that the frontal lobes are an attention-center. Against this assumption it is urged that it accounts for nothing which can not be as well explained 'on the hypothesis that the organ of attention is the cortex as a whole'; but though the argument is well sustained it does not connect itself closely with the experimental results.

As a whole, the paper is an instructive illustration of the value of the experimental results which may be obtained without the aid of laboratory or of apparatus, by an investigator who is quick to apprehend a problem, accurate in defining it and ingenious in methods of working it out.

Zur Kenntniss der nachlaufenden Bilder. A. SAMOJLOFF. *Zeitschrift f. Psychol.*, XX., 2 and 3.

Samojloff experimented, at von Kries's suggestion, on the after-images, from morning light-stimuli, with especial reference to the points at issue between von Kries and Hess: the color of the after-image and its relation to stimulation of the center of clearest vision. The method and apparatus of the older experiments were completely set aside, in order to avoid the sources of possible error suggested by Hess. The color stimuli were given through openings of a revolving desk, which formed the front of a 'dark box' whose degree of illumination could be regulated. The results confirmed von Kries's conclusions; the after-images of the yellow stimulus were blue, and those of the blue were yellowish, that is, the after-images were negative, and not in accordance with Hess's results. Positive experiments on the stimulation of the center of vision also show results similar to those of the earlier Freiburg experiment: in the vivid, even though inexact, words of Samojloff "the after-image overleaps the central region around the fixation-point." It is shown that this is not a mere case of obliteration of an after-image through the brightness of the fixated point, for an equally intense light, illuminating the *periphery* of the retina does not annihilate the after-image.

The writer calls attention to the close correspondence of his results with von Kries's theory that the after-image depends upon the activity of the '*Dunkelapparat*' which is wanting in the center of vision; yet he does not claim that these experiments furnish 'rigid proof' of the entire lack of the '*Dunkelapparat*' in this part of the retina.

M. W. CALKINS.

WELLESLEY COLLEGE.

PATHOLOGY AND NEUROLOGY.

L'Instabilité Mentale. Essai sur les donnés de la psycho-pathologie. G. L. DUPRAT. Paris, Alcan, 1898. 8vo. Pp. 310.

The motive of M. Duprat's book is not so much psychological as philosophical: his intention, in his own words, is less to write a book of science than to consider scientific conclusions and to examine the

first principles of the science with which he deals in order to give it, if possible, a philosophic foundation. In pursuance of this plan M. Duprat attempts to show the primacy which psychology has over physiology in the study of mental pathology. The more particular purpose of the book is to emphasize the importance of the concept 'mental instability' in psychical disease, and to relate all concrete mental maladies to this as species to genus. No mental process can normally occur unless there exist a principle directing the mental evolution, which by its permanence resists the natural instability of the mind. The more feeble the principle, the greater the distraction. Duprat's book, then, occupies itself with collecting the various medico-psychological observations upon the diverse forms of psychopathy, and with discovering in each of these forms a foundation of psychological instability. In pursuance of this purely philosophical plan the book is divided into three parts.

The first part, a general introduction to the rest, is concerned with the mental processes as a whole, normal and abnormal, and attempts to show that biology can go only part of the way in psychiatry, and that psychology must do the greater part of the work. It is impossible to deny the existence of biological disturbances underlying psychic ones, but there may at the same time be purely psychological causes of psychopathies. The biologic centers are also psychic centers.

The second part occupies itself with the consideration of the various psychopathic symptoms in detail, and attempts to find in each the fundamental fact of mental instability. This root-malady is classed according to its four aspects: instability of the intellect—incoherent thought; instability of the tendencies—the illogical rise of one from another; instability of the feelings—the rapid alternation from love to hate, etc.; and instability as action—aboulia, ataxia, etc. In the same part of his work, after considering the particular mental diseases specifically, M. Duprat considers them as a whole, under the title 'pathology of personality,' and the alternations of this general psychopathy according to sex, habit of life, and age. Here is included marked mental stability—the stubbornness of melancholia, for instance—which is shown itself to be rooted in the more fundamental disturbance of mental instability.

The third and last part of the book is occupied with the practical conclusions resulting from these conclusions.

The value of M. Duprat's book, as he himself admits, is purely philosophic, and can have interest only for those interested in attempts at logical classifications, and the inclusion of specific concepts under

one concept embracing them. This being the case, it is to be regretted that M. Duprat has been unable to define his class concept of mental instability in any definite way; so that after following all the concrete mental pathologies through M. Duprat's close-written pages, and learning that they are explained by one inclusive concept—mental instability—we are compelled to ask, what is mental instability?

D. P. BARNITZ.

HARVARD UNIVERSITY.

Zur Theorie der Nerventhätigkeit. Professor EWALD HERING ('Akademische Vortrag'). Leipzig, 1899. Pp. 31.

This little pamphlet of Professor Hering, while not the product of actual experimental research, has interest and value because it represents the opinion of a man who is, from his broad outlook, most competent to judge in a case so long and actively controverted as is this one. The writer in substance upholds the doctrine of the specific energies of the various parts of the neural organism, following therein especially J. Müller, but he goes further (as the rise of the neuron-theory necessitates), and strongly believes that not the cells of the neurons only, but also the prolongations from these have forms of nervous activity peculiar to themselves and to their respective uses in the organism. "The activity of the neuron and of its fibers," he says "may depend not alone, as some think, on the intensity but also on the quality of its sort of stimulus, whether this come from its own peripheral sense-organ or from a neighboring neuron." The physical basis of the difference in function is deemed to consist in the various forms of neural vibration which, with indefinite differences in the neural substance chemically, is emphasized as the 'inheritance' of the neuron and its projections.

GEORGE V. N. DEARBORN.

COLUMBIA UNIVERSITY.

NEW BOOKS.

Psychology and Life. HUGO MÜNSTERBERG. Boston, Houghton, Mifflin & Co., 1899. Pp. xiv + 282.

Criteriologie Générale, ou Théorie générale de la certitude. D. MERCIER. Louvain, Inst. Super. de Philosophie. Pp. v + 371. 6 Fr.

Discorsi su la Natura e sul Governo dei Popoli. F. P. C. SIRAGUSA. Palermo, Virzi, 1899. Pp. 410. L. 5.

The Messages of the Earlier Prophets. F. K. SANDERS and C. F. KENT. Second ed., New York, Scribners, 1899. \$1.25, net.

The Psychology of Reasoning. A. BINET. Trans. from 2d French edition by A. G. WHYTE. Chicago, Open Court Co., 1899. Pp. 191.

It is well to have in English this new edition of Professor Binet's well-known book—one of the first publications of this prominent French psychologist. Its positions are too well known to require statement. The translation is very well done. J. M. B.

Die Philosophie der Geschichte als Sociologie. P. BARTH. Erster Teil, Leipzig, Reisland, 1897. Pp. xii + 396.

Mainly a historico-critical review of sociological theories. Extended notice of this important work is reserved until the appearance of the later parts. J. M. B.

La Psicogenesi della Istinto e della morale secondo C. Darwin. P. SCIASCIA. Palermo, Reber, 1899. Pp. xv + 178. L. 4.

Talks to Teachers on Psychology and to Students on some of Life's Ideals. WM. JAMES. New York, Holt, 1899. Pp. xi + 301.

Marriages of the Deaf in America. E. A. FOY. Washington, GIBSON for Nolta Bureau, 1898. Pp. vii + 527.

A valuable statistical study, with conclusions on the inheritance of deafness, etc., having important general bearings. J. M. B.

Personal Competition. C. H. COOLEY. Vol. IV., No. 2 of Economic Studies, American Economic Association. New York, Macmillans, 1899. Pp. 173.

Les Transformations du pouvoir. G. TARDE. Paris, Alcan, 1899. Pp. x + 266.

Wörterbuch der philosophischen Begriffe und Ausdrücke. R. EISLER. Dritte Lieferung, Empfindung to Geschichtsphilosophie. Berlin, 1899. M. 2.

As this important *Wörterbuch* proceeds, both its excellences and its defects appear. It is made up mainly of citations under each head of definitions by various authors. It attempts no critical or definitive settling of meanings. It gives no equivalents in other languages. Its greatest defect is its extraordinary limitation in the matter of literary citation—limitation to German sources. Of English and American writers since Hamilton and Spencer, we have noticed in the psychological articles of the three first *Lieferungen*: one reference to James, one to Stout, one to Baldwin, and none to any other English or American writer except Bain; and this, after looking up several of the most

important psychological topics. The compiler seems limited, in his citations of both French and English authors, to works which have been translated into German. Wundt is the authority quoted under all the headings. When completed, the work, which is a perfect mine of citation from German writers, will be given full notice in the REVIEW.

J. M. B.

Nervous and Mental Diseases. H. CHURCH and F. PETERSON. Philadelphia, W. B. Saunders, 1899.

A remarkably able and valuable compendium. The Neurology is written by Dr. Church and the Psychology by Dr. Peterson. It is fully illustrated and the cuts of apparatus have great interest to the psychologist, to whom indeed the entire book should prove of very great value. We hope to print a detailed expert review.

J. M. B.

The Metaphor: A Study in the Psychology of Rhetoric. G. BUCK. Inland Press, Ann Arbor, Michigan, no date.

Geschichte des Lebensmagnetismus und des Hypnotismus von den ältesten Zeiten bis auf die Gegenwart. H. R. P. SCHROEDER. In 12 Lieferunden. Parts I.-V. Leipzig, Strauch, 1899. Parts M. 1 each.

Das Hypnotische Hellseh-Experiment in Dienste der naturwissenschaftlichen Seelenforschung. R. MÜLLER. I. Band, das Veränderungsgesetz; Band II., das normale Bewusstein. Leipzig, Strauch, 1899. Pp. viii + 168, and 169-322. M. 5 and 4.

Bewusstsein und Hirnlokalization. W. v. BECHTEREW. Deutsch von R. WEINBERG. Leipzig, Georgi, 1898. Pp. 50. M. 1.50.

Suggestion und ihre sociale Bedeutung. W. v. BECHTEREW. Deutsch von R. WEINBERG. Vorwort von P. FLECHSIG. Leipzig, Georgi, 1899. Pp. iv + 84.

Mathematical Contributions to the Theory of Evolution, VI. Genetic (Reproductive) Selection. Inheritance of Fertility in Man and of Fecundity in Thoroughbred Race Horses. K. PEARSON, A. LEE, and L. BRAMLEY-MOORE. Philos. Trans. Roy. Society of London; London, Dolan & Co., 1899. 3s. 6d.

Des Religions Comparées, au point de vue Sociologique. R. DE LA GRASSERIE. Paris, Giard et Brière, 1899. Bib. sociologique intern., No. xvii. Pp. 396. 9 fr. or 7 fr.

Interpretation Sociale et Morale des Principes du Développement Mental. J. MARK BALDWIN. Paris, Giard et Briere, 1899. Bib. sociologique intern., No. xviii. Pp. vi + 580. 12 fr. or 10 fr.

Naturalism and Agnosticism. JAMES WARD. Gifford Lectures, Aberdeen, 1896-1898. London and New York, 1899. Pp. xviii + 302 and xiii + 294. \$4.

NOTES.

DR. A. E. LOVEJOY has been appointed assistant professor of philosophy in Stanford University.

From Comte to Benjamin Kidd; the Appeal to Biology or Evolution for Human Guidance is the title of a book by Robert Mackintosh, to be published immediately by The Macmillan Company.

PROFESSOR A. H. KEENE, F.R.G.S., late Vice-President of the Anthropological Institute of London, has written a work on *Man, Past and Present*, which will be published in the United States by The Macmillan Company.

DR. D. S. MILLER, formerly of Bryn Mawr College, is to give courses (see the Harvard 'Announcement' in this issue of the REVIEW) in the Harvard Philosophical Department during the coming year, Professor James being away on his 'Sabbatical' vacation.

WE regret to record the death of Professor Ludwig Strümpel, of Leipzig. He had reached the age of 87 years.

AMONG psychologists and philosophers summering abroad we note President Patton and Professors Gardiner, Howison, Bliss.

PROFESSOR JASTROW is to return to his work in the University of Wisconsin in September.

ASSISTANT PROFESSOR F. KENNEDY has been made full Professor of Philosophy in the University of Colorado.

P. H. HORNE, a graduate of and instructor in the University of North Carolina, has been appointed instructor in the department of philosophy in Dartmouth College.

ON page 288 of the May number of the REVIEW, line 2 should be 'asserts that there *is* an instinctive fear of a cat.' The title of the article should be 'The Instinctive Reactions of Young Chicks.' On page 286, in the last line, 'prooning' should be 'preening.'

EDWARD THORNDIKE.

